John Cranford,
M. D.

MEDICAL FACTS

AND

OBSERVATIONS.

VOL. III.

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SCHOOL OF MEDICINE

MEDICAL FACTS

AND

OBSERVATIONS.

I. *Cases of Ischuria Renalis in Children.* By Robert Willan, *M. D. F. A. S. Physician to the Public Dispensary in London.*

IN the course of the year 1784 I observed three instances of sudden death in children who had not been previously affected with any violent or alarming complaint.

The train of symptoms was nearly as follows: —At first a slight feverish heat, restlessness, diarrhœa, and sometimes bilious vomiting, which continued for about a week; during that time the urine was made in small quantity, till at length the discharge of it entirely ceased, and soon afterward the patients died unexpectedly,

without complaining of pain or any particular uneaſineſs.

No medical aſſiſtance having been thought requiſite in the firſt days of the diſeaſe, I did not, therefore, ſee any of theſe caſes till the ſuppreſſion of urine had taken place, and at that time found the diarrhœa abated, the pulſe and ſtate of the ſkin natural.

The predominant ſymptom, iſchuria, was the only one which demanded immediate attention. As there appeared no painful tenſion or ſwelling in the hypogaſtric region, nor in any part of the abdomen, I had hopes of affording ſpeedy relief by means of cooling diuretics, clyſters, and fomentations.

Theſe applications, however, and other remedies which the circumſtances ſuggeſted, were inſufficient to reſtore the diſcharge of urine before the patient fell a victim to the diſeaſe.

To be thus diſappointed in ſo many caſes, occurring nearly together, made me very uneaſy. I found no ſatisfactory information in medical authors, nor from practitioners with whom I converſed on the ſubject: I determined, however, if a ſimilar caſe ſhould again occur, to employ diligently the ſemicupium, or

warm bath, as a remedy moſt generally efficacious in the iſchuria renalis.

Two years afterwards an opportunity occurred of adopting this plan in the caſe of Maſter K., a fine ſenſible boy about nine years old. In the beginning of October, 1786, he had the ſcarlatina anginoſa, of which he ſoon recovered under the care of Mr. Thomas Auſtin, his apothecary. He continued perfectly well for about a week, went out every day, and amuſed himſelf as uſual. It was then obſerved that he began to loſe his appetite, and to be ſomewhat debilitated: he had alſo frequent, though ſlight, attacks of ſickneſs and diarrhœa.

On the 23d and 24th of October he ſeemed much better; but on the 25th found a total inability to make water.

Mr. Auſtin, to whom I had formerly communicated my obſervations reſpecting the inſidious nature of theſe complaints in young ſubjects, deſired me to ſee this patient early on the 26th. I found him then eaſy and compoſed: his ſkin was cool, his pulſe at 90, and he had no mark of fever, except a ſlight yellowiſh-brown fur on the back part of the tongue. His cheeks were

rather bloated; but he had no pain of the head or ſtomach: his abdomen was not in any part tenſe or ſwelled, nor had he any ſenſe of pain on its being preſſed.

After preſcribing a gentle laxative, I recommended the warm bath as the remedy to be principally truſted.

He was kept in the bath, on the firſt application of it, twenty minutes. Afterwards, as he ſtood on the floor, he made a ſmall glaſsful of water. No internal miſchief could be detected from the ſtate of this urine, which was clear and of a natural colour.

In the evening he was briſk and lively: his cheeks did not appear ſo much bloated. I directed that the bath ſhould be again employed, hoping it might produce an effect as great as before, or even greater.

Next morning he was nearly in the ſame ſtate; but he had made only a few drops of water on coming out of the bath. I then deſired he might be kept in it three quarters of an hour, and attempt to make water in that ſituation, or afterwards ſtanding on the cold floor.

At night I was informed that theſe directions had been attended with little ſucceſs; and farther, that during the day he had been ſeized with

with a kind of fit, in which, after a ſudden ſhivering, he became very cold and inſenſible, having his eyes fixed for ſome minutes. He ſoon, however, recovered from this ſtate, and when I ſaw him again was ſerene and cheerful as on the preceding day.

Though the ſymptoms were not aggravated ſince my firſt viſit, ſtill he was evidently in a very precarious ſituation. So much watery fluid could not be retained in the blood, the urinary diſcharge being ſuppreſſed, without inducing fatal conſequences from its effuſion on the brain, a circumſtance ſo frequent in ſimilar caſes.

A conſultation was propoſed, to which I could not but readily accede: this was, however, deferred till next day, it being then late in the evening. I deſired, in the mean time, that a fourth trial might be made of the warm bath.

At eleven o'clock he had ſtrength enough to get in and out of the bath without aſſiſtance. He then went to bed, and talked cheerfully to thoſe about him till twelve o'clock, when he ſuddenly complained that he could not ſee, and very ſoon after expired in a fit ſimilar to that with which he had been affected in the courſe of the day.

An examination of the diſeaſed parts not being permitted, I could not thence confirm or diſprove the ideas I had formed reſpecting the complaint from the preſent caſe.

Another inſtance of the ſame kind occurred March 20th, 1787, in a child about two years old. — His mother informed me, that ſix days before he had been taken with a diarrhœa and vomiting. Thoſe ſymptoms abated within a day or two, but a cough and ſome degree of fever ſtill remained. The diſcharge of urine had ceaſed upwards of twenty four hours before he was brought to me: he was then very reſtleſs; his breath was ſhort; his pulſe ſmall and quick; his face was bloated, and of a ſallow or almoſt livid hue; but there appeared no hardneſs or ſwelling of the abdomen.

As the caſe was evidently in its extreme ſtate, I deemed it unneceſſary to direct the application of any remedies, and preſently learnt that the child died within an hour after its return home.

Being allowed to inſpect the viſcera of this patient, I at length diſcovered the ſtate of the parts affected in this dangerous malady. The whole of the meſentery appeared to have been inflamed.

inflamed. The inflammation had extended thence to a conſiderable portion of the ileum, on which were alſo found two gangrenous ſpots, each of them about the ſize of a ſixpence.

There was no urine in the bladder: the ſtomach, liver, and kidnies, were in a natural ſtate; the gall bladder was very turgid, and the meſenteric glands were much enlarged.

In the courſe of my attendance on the above patients I had been led to conclude that the ſymptoms aroſe from an inflammation of ſome of the abdominal viſcera. The laſt caſe, which confirmed that opinion, alſo limited the ſeat of inflammation to the meſentery. The circumſtances appearing in all of them were ſo ſimilar, that they might reaſonably be referred to the ſame cauſe, and probably the common termination was by gangrene; at leaſt I am inclined to think this muſt have happened to Maſter K., at an early period, even before my firſt viſit to him, which is rendered more probable from his having had violent ſhiverings, with coldneſs and inſenſibility, for a conſiderable time, on the 22d of October, when the com-

plaint in his bowels firſt abated, a circumſtance of which I was not informed till afterwards.

It farther appears, from all theſe caſes, that meſenteric inflammation has the ſame ſlow and obſcure courſe of ſymptoms as inflammation affecting the omentum, peritonæum, and mediaſtinum.

The beſt account of meſenteritis is given by Riverius*. It coincides in moſt particulars with the caſes which I have related above; but neither Riverius nor any other author has mentioned the renal iſchuria as a ſymptom of the diſorder.

On comparing together the caſes above related, I was induced to think that the ſuppreſſion of urine only came on after gangrene had taken place; but another caſe which has ſince occurred proves the contrary.

* "Signa inflammationis meſenterii diagnoſtica ſunt "febris languida, occulta et lenta, abſque ſiti & gravioribus "ſymptomatis: anorexia; tenſionis & gravitatis ſenſus "ſubter ventriculum, citra magnam duritiem & quæ non "niſi preſſu dignoſcitur: citra dolorem etiam, ſaltem ef- "fatu dignum, quia pars obtuſo ſenſu donata eſt: ejec- "tiones chyloſæ quas plerumque ichor tenuis conſequitur "ſine ullo doloris ſenſu, modo ſeorſim & ſincerus, modo "fæcibus permixtus." — *Vide* Riverii Praxeos Med. Lib. xiii. cap. 2.

A child

A child, three years old, was brought to the Publick Diſpenſary on the 28th of September, 1789. This patient had made no urine for upwards of eighteen hours. The prior ſymptóms had been nearly ſimilar to thoſe mentioned in the foregoing accounts. He was uneaſy and fretful, had a quick ſmall pulſe, but no fur upon the tongue, nor any degree of tenſion in the abdomen.

I ordered eight leeches to be applied to the abdomen, and a bliſter near the os ſacrum; and was informed on the following day that the urinary ſecretion had been fully reſtored. The diſcharge continued afterwards in regular order, and the ſtate of his bowels was natural. His original diſeaſe, therefore, appeared to be completely removed; but in the courſe of a few days he again became languid and heavy; and I am ſorry to add, that he died about a month after with ſymptoms of hydrocephalus diſtinctly marked.

As the above caſes all occurred in infants or young children, it might ſeem probable that iſchuria, as a ſymptom of meſenteric inflammation, is peculiar to them; but this concluſion would not be juſt, as I have ſeen one fatal caſe

case of the same kind at adult age, attended with the usual insidious train of symptoms.

II. *A Case of Pemphigus. By* T. M. Winterbottom, *M. D. Physician to the Settlement at Sierra Leone.*

MR. Y., aged eighteen years, of a sallow complexion, but healthy, in the summer of 1790, made a voyage to Archangel. On his arrival at that place he was, without any previous uneasiness, affected with an eruption of small vesicles, of the size of pease, which gradually increased to the size of large hazle nuts: they continued in this form for some time, then burst, and discharged a thin fluid of a light yellow colour, like an infusion of green tea, leaving the skin beneath excoriated, and in a very sensible state. These vesicles affected in particular his face and legs; they did not observe any regular period, but as one set of them disappeared others broke out afresh.

These successive eruptions continued during the whole time of his stay at Archangel, and he was not entirely free from them until he had passed the North Cape on his passage home. The only application made use of was a little cerate, to prevent the cloths from sticking to the

the wounds, and he took a doſe or two of Glauber's ſalt.

But the peculiarity of this caſe is, that the very ſame train of ſymptoms took place upon a ſecond voyage to Archangel in 1791. The veſicles came out in like manner, making their appearance in ſucceſſion; nor did the eruption ceaſe till he had again paſſed the North Cape.

When I firſt ſaw him, which was ſome months after this ſecond voyage, his face and legs were covered with ſpots nearly the ſize of a ſixpence, in colour reſembling the marks left by the ſmall pox; many of theſe were attended with conſiderable depreſſions of the ſkin, intomuch as to produce a ſuſpicion, among perſons not much acquainted with the diſeaſe, of his really having had the ſmall pox. The diſcolouration of the ſkin was not entirely removed for a twelvemonth after the diſeaſe had left him, but was ſtill very evident in cold weather. None of the people in the ſhip with him were affected in the ſame manner; neither could the complaint be referred to the bites of muſquitoes or other inſects, ſince he had been at St. Peterſburgh and other places equally infeſted with them without experiencing the leaſt inconvenience.

III. *Caſe*

III. *Case of Injury of the Brain, without a Fracture, relieved by Application of the Trephine. By Mr.* John Andrews, *Surgeon in London.*

ON the 23d of July, 1791, Elizabeth Bell, of Great White Lion Street, thirty-five years of age, rather corpulent, and of a sanguineous temperament, fell down eight or ten stairs, and struck her head upon a boarded floor.

She remained without any medical assistance until the third day after the accident, when I was desired to see her. The bystanders informed me that she had become sick and speechless immediately after the fall, and, after a short interval, comatose. She now discharged her urine involuntarily, and had had a shivering fit a few hours before I saw her: twelve ounces of blood were, therefore, taken from the arm, and a solution of sal. cath. amar. was directed to be given till the bowels were emptied; her whole head was also ordered to be shaven, and a blister was applied to the posterior part of the neck.

On the day following, as her former symptoms were not at all relieved, I thought it advisable to open the temporal artery, which was done

done, and with ſome advantage; for her pulſe, which before was very ſlow and oppreſſed, beat a little quicker before I left her, and upon being rouſed, and aſked how ſhe did, ſhe replied, "Better than before." As the cathartic medicine had produced no effect, a purging enema was adminiſtered in the afternoon, and ordered to be repeated, if neceſſary.

The whole head was now more attentively examined both by myſelf and my friend Mr. Blair, who was preſent; but no injury either of the ſcalp or cranium could be diſcovered, though we preſſed pretty forcibly on every part of the head.

On the ſubſequent morning (28th) I was informed ſhe had been attacked in the night with convulſive fits, two of which occurred while I was with her. I obſerved that only the left ſide of her body was affected, her mouth and face on that ſide being diſtorted as in epilepſy; that her head inclined to the left ſide; and that the leg and arm on the ſame ſide (particularly the latter) were contracted and violently agitated.

During this day ſhe had frequent returns of the convulſions, each fit laſting only a few minutes: I therefore drew ten ounces more blood from the arm, and reſolved, if this did not relieve

lieve her, to divide the ſcalp on the following day, and ſearch more minutely for a fracture. Upon preſſing the ſcalp with my finger, I obſerved at this time that a little depreſſion was left; but, as this was not a circumſcribed œdema, it could not be regarded as a ſufficient reaſon for making an inciſion, or applying the trephine on any particular part.

The œdema was more univerſal on the ſubſequent morning (the 29th): the convulſions were become frequent and violent; her pulſe was ſlow and more depreſſed; her fæces and urine were ſtill diſcharged involuntarily, and her extremities were becoming very cold. Theſe alarming ſymptoms ſeemed to juſtify a mode of treatment that would otherwiſe not have been adopted: I therefore made an inciſion three or four inches in length on the left parietal and temporal bones; but as no injury was perceived on the denuded bones, the lips of the wound were placed in contact, and kept together by means of ſticking plaſter, in order to accelerate their union. A free inciſion, ſimilar to the former, was then made on the oppoſite ſide, and the bone laid bare, but no preternatural appearance was obſervable, though, on preſſure upon the anterior and inferior edge of the parietal bone, the

the patient ſeemed to ſuffer pain, as I judged from her drawing up the right angle of the mouth. Upon this part of the cranium, and upon the edge of the ſquamous ſuture, I applied the trephine. As ſoon as the circular piece of bone was removed, a quantity of extravaſated blood (which I ſuppoſed from its colour to be venous) was ſeen through the dura mater, diſtending that membrane: a ſmall oblique puncture was, therefore, made with a lancet, and a table ſpoonful of blood was diſcharged by a ſtream. A ſmall piece of lint was then applied lightly on the dura mater, and the wound dreſſed in the uſual way. After the operation her pulſe, which before had been very ſlow and oppreſſed, quickened, and in a few hours beat 120 in a minute. She was ordered to take an opening mixture occaſionally, and ſtrict attention was paid to her diet.

On the 30th of July, the day after the operation, ſhe had ſeveral more fits, but they were leſs violent and recurred leſs frequently than before; the ſame quickneſs of pulſe continued, and ſhe ſlept better than uſual.

On the 31ſt, on account of the fulneſs of her pulſe, eight ounces of blood were taken from the

the arm: she had this day only two fits, and was much relieved from her other complaints.

She continued to get better, nothing very particular happening, until the 4th of August, when, upon removing the dressings, a remarkable elastic tumour was discovered on the left side of the head, extending about three inches in length, and two or more inches in breadth. What could be the cause of this swelling? If it had been occasioned by an effusion of blood under the scalp, would it not have appeared much earlier?—Mr. Pott used to regard such a puffy rising of the integuments as a pretty sure criterion of a fluid under the cranium, immediately beneath the swelling. But whatever might be the cause of this appearance, it did not remain long; for in little more than a week it gradually disappeared. The granulations from the dura mater were now uniting with those from the scalp, and by the 6th of October the wounds were perfectly healed.

The last time I saw her (December 10th) she was as entirely free from complaint as she ever had been at any time previous to the accident.

Great Russell Street, Bedford Square,
Feb. 22, 1792.

IV. *Case*

IV. *Case of a Cyst containing Hydatids, extracted from the right anterior Ventricle of the Brain of a Cow. Communicated in a Letter to Dr.* Simmons *by Mr.* William Moorcroft, *Veterinarian Surgeon in London.*

To Dr. Simmons.

SIR,

PRESUMING on the favourable reception which every attempt to improve the long-neglected study of the diseases of domestic animals appears to experience at present from the Public in general, but more particularly from medical practitioners, I am induced to request the insertion of the following case in the next volume of Medical Facts and Observations. Though it cannot be considered as containing any material practical improvement, yet its communication may be useful, inasmuch as it may excite a degree of more general attention to an object which has hitherto been very partially understood.

As we have few cases of this nature upon record, and as this may, on that account, become an object of occasional practical reference, I have entered into details which, perhaps, may

appear trifling or ſuperfluous to many medical readers, but which notwithſtanding may be of ſome importance to thoſe who are more particularly intereſted in the ſubject.

I remain, SIR,

Your very obedient, humble Servant,

W. MOORCROFT.

CASE.

It has been long known to anatomiſts, but more particularly to ſhepherds and butchers, that collections of a tranſparent colourleſs fluid, in one or more ſemi-opaque, thin, membranous capſules, generally of a ſpheroide figure, and of different ſizes, are occaſionally to be met with on the ſurface or within the ſubſtance of the brain of ſheep, particularly of thoſe of a certain age; and experience has uniformly proved that ſuch a collection conſtantly produces the death of the animal in which it is found, if it be left entirely to itſelf: yet notwithſtanding the ſymptoms characteriſing the preſence of ſuch a collection have been aſcertained by thoſe converſant with theſe animals, and in conſequence attempts have been made to relieve the affected animals, both by medical practitioners and others,

others, it is yet to be regretted that the Public of this country have not been benefited by the communication either of the diagnoſtic ſymptoms, or of the reſults of the different experiments. It is true ſome old authors, in treatiſes on rural œconomy, have mentioned this circumſtance; but this has been done in ſo looſe a manner, that little, if any, dependence is to be placed upon the obſervations they have tranſmitted us.

Sheep are not the only domeſtic animals which are incident to theſe collections, though they appear to be more particularly ſo than any other claſs: cows are next moſt frequently ſubject to them, and they have been met with in dogs; but I am not acquainted, either from experience or record, with the occurrence of a ſimilar circumſtance in horſes, though the preſence of ſmall globular bodies in the plexus choroides of theſe animals is far from being unfrequent; but theſe have always appeared to me to bear a greater reſemblance to the *ſimple* * than

* By *ſimple hydatid* is meant an accumulation of water in a capſule compoſed of conſolidated laminæ of cellular membrane. By *animal hydatid* is underſtood a veſicular worm, or organiſed body, which enjoys life diſtinctly from that of

than to the *animal hydatid*, the ſubject of this paper.

The circumſtance of an animal having ſuch a collection within the cavity of the cranium, frequently turning in a circular direction, appears to have been conſidered as the characteriſtic, unequivocal ſymptom, and accordingly we find the diſeaſe, produced by the collection, called, in ſome parts of this country, the *gid*, in others, *turn*; in France, *tournoiement*, or *vertige*; in Italy, *ſtorno*, or *male vertiginoſo*. But as I am not yet in poſſeſſion of a ſufficient number of facts to enable me to give a ſatisfactory account of this diſeaſe, I ſhall confine myſelf to the relation of the caſe of a cow, in which I lately met with it.

Being at Ormſkirk, in Lancaſhire, in the beginning of September, 1791, I was deſired to ſee a cow, whoſe diſeaſe had baffled the endeavours of every one who had attempted to relieve her. She was a two-year-old heifer, of the long-horned or Lancaſhire breed, and had been

the animal in which it is evolved, and which contains, or is contained in, a certain quantity of fluid.

There are many ſpecies of this worm, which have been named in alluſion to their mode of life, form, &c., as *ſolitary*, *ſocial*, *tæniaform*, *piſiform*, *aciniform*, &c.

always, until attacked by the preſent complaint, in apparently good health, and tolerable condition. On the 1ſt of May, 1791, ſhe was put to graſs in a ſcore at ſome diſtance from the reſidence of the proprietor, which prevented his ſeeing her for ſix weeks, when he found her much worſe in condition, and continually rambling about the field without appearing diſpoſed to eat. That ſhe might be under his immediate care, he had her removed to a little cloſe near his own houſe, and made uſe of the different means pointed out by thoſe he conſulted. Here ſhe remained till I ſaw her, without having been apparently benefited in the leaſt degree by any thing which had been done for her.

I found the motions of her limbs in walking unuſually ſlow, languid, and unſteady; her belly tucked up, her flank hollow, and, in ſhort, her whole appearance announcing a ſtate of conſiderable emaciation and debility; yet, notwithſtanding, ſhe appeared to have a deſire for food: her pulſe was rather weak, but regular; her reſpiration natural, and the common evacuations were ſuch as occur in a healthy ſtate, except that the quantity was rather diminiſhed.

It was obſerved that ſhe always carried her head very near the ground and was continually

engaged either in walking near the hedges, or turning in a circular direction. On attending more closely to this circumstance, I remarked, that when driven towards the right hand, she constantly formed a circle of about three yards in diameter; and that, on the contrary, when tempted to go to the left, she kept at a little distance from, and followed the line of, the hedges and ditches, and occasionally thrust her head, and particularly the right side of it, against projecting boughs and tall tufts of grass.

She had been so long accustomed to ramble in a slow, but almost continual, walk, in the way just mentioned, that the field exhibited some unusual appearances; in the middle the grass was trodden perfectly flat, in nearly equally sized circular paths of about eighteen inches in breadth, and of the diameter before mentioned, whilst, on the contrary, on the sides there was only one path at about a yard from the fence, which followed exactly its different directions, and observed generally the same breadth and distance.

The head of the animal was constantly held obliquely downwards, so that the left horn was considerably more elevated than the right. On examining the head in general, I was not sensible of the smallest morbid alteration either in feel

feel or figure, but remarked that the right noſtril appeared to yield a larger quantity of mucus, and the lining membrane to be ſomewhat redder than that of the left. The eyes were heavy, from the eye-lids being more cloſed than is uſual; but I was not aware at this time of any diſeaſed appearance in either of the pupils. The intervals from rambling, which were but of ſhort duration, were employed in eating rather greedily.

On taking theſe different circumſtances into conſideration, I was induced to believe that the preſence of one or more of the larvæ of the œſtrus * in one or more of the naſal cavities might be the cauſe of theſe ſymptoms, and in conſequence of this idea removed a circular piece of bone

* A fly well known to inſectologiſts, and to be met with almoſt every where in the country in the autumnal months, but particularly in the neighbourhood of woods. It comes under the claſs *Diptera* of Linnæus:

"Alæ duæ. Halteres clavati, ſolitarii pone ſingulam "alam ſub ſquamula propria.

"*Œſtrus*. Os nullum, punctis tribus, abſque Proboſcide "aut Roſtro exſerto."—Vide Car. Linnæi Entomol. cur. Car. de Villers. 8vo. Lugdun. 1789. Tom. III. p. 345.

It ſhould ſeem that this fly is a true paraſite, and ſtands in need of an animal nidus during the ſtates of egg and larva. Some ſpecies *(œſtrus bovis)* depoſit their eggs in the ſkin

bone from the upper part of the naſal, and another from the moſt depending part of the right maxillary ſinus. The perfectly ſound ſtate of the membrane of theſe cavities convinced me of my error, and after being fully ſatisfied that the inflammation with which I was ſtruck was only confined to a very ſmall, and that the lower, portion, and probably produced by ſome external cauſe, I brought the flaps of ſkin into contact, and quitted the animal. This operation did not produce the ſmalleſt change in the ſymptoms; union of the divided tegument took place, and I loſt ſight of the animal till the latter end of the month, when the proprietor informed me that other aſſiſtance had been had recourſe to, but without advantage, and that, unleſs I adviſed the trial of ſome farther means, he would have her killed, that I might have an

of the backs of horned cattle and rein deer; others *(œſtrus naſalis)* in the naſal cavities of ruminating animals, but particularly of ſheep, and occaſionally in the interſtices of the folds of the lateral and upper part of the pharynx of the common or fallow deer; others again (improperly called *œſtrus hæmorrhoidalis)* lay their eggs upon the hairs of the ſides of horſes, ſome of which are occaſionally ſwallowed, and produce the larvæ commonly called botts, and which obtain in greater or leſs number in the ſtomach of almoſt every horſe.

opportunity

opportunity of being ſatisfied by diſſection as to the cauſe of the complaint. Unwilling to have this done without reviewing the ſymptoms, I re-examined the head with the greateſt accuracy, and found the pupil of the right eye more dilated than that of the left, though the latter was larger than it ought to have been; and though the former was not deſtitute of irritability, yet the latter enjoyed a greater latitude of motion.

From the addition of this to the former ſymptoms, I began to imagine that there might be an hydatid either upon the ſurface or within the ſubſtance of the brain, and was inclined to believe, that if ſuch was the caſe, there was a greater probability of detecting it by perforating that part of the ſkull correſponding with the right hemiſphere, than elſewhere.

The owner of the cow having conſented to whatever experiment or operation I might propoſe, I had her caſt, and ſecured by means of fetters, and made a crucial inciſion through the ſkin upon the right frontal bone, on a level with the ſuperior part of the orbit, and very near the longitudinal future. After detaching the pericranium, I applied a trephine of nearly an inch diameter, and having removed two circular pieces of bone, I cut off the intercircular angles,

ſo

ſo as to reduce the whole to a longitudinal oval opening of ſomewhat more than two inches in length and one in breadth. Not meeting with any thing extraordinary upon the ſurface of the dura mater, I placed the ſcalpel upon it, in order to make an opening in it parallel with that of the bone, and was not a little ſurpriſed to find it oſſified; I, however, cut out a piece of it correſponding with the opening in the bone.

The veſſels of the pia mater appeared almoſt obliterated, or nearly colourleſs, from their containing an unuſually ſmall quantity of blood, and the expoſed ſurface of the brain preſented two conſiderable eminences ſeparated by a furrow, in which was a colourleſs vein which acted as a band, but yet not ſufficiently ſo as to prevent theſe eminences preſſing rudely againſt the edges of the bone.

On preſſing my finger on the brain, I received the ſenſation of fluid reſiſtance from within, and began a longitudinal inciſion in the upper and middle part of the denuded portion. The cortical part cut as if much upon the ſtretch, and was thinner and harder than uſual; on dividing the vein juſt mentioned, the lower part began to tear before the knife, and, when the whole inciſion was effected, the preſſure from within

within became ſo conſiderable as to render the farther uſe of the cutting edge of the knife entirely unneceſſary. By cautiouſly ſeparating the divided edges with the handle of the inſtrument, I was ſtruck with the appearance of a cyſt, part of which protruded itſelf immediately with conſiderable force, through the bony opening, to the ſize of a hen's egg, when it burſt, and gave iſſue to about three or four ounces of a thin colourleſs fluid. By laying hold of the torn edges, and drawing them gently from one ſide to the other, I detached the cyſt from its connections without the leaſt degree of laceration, except what was before produced by the eſcape of the contents.

On looking into the brain, after the removal of the cyſt, I was only aware of a large cavity, the ſurface of which was perfectly ſmooth and white, not containing any more hydatids, nor the ſmalleſt quantity of water. From the preſence of the plexus choroides, which lay at the bottom, though much paler coloured and ſmaller than uſual, I concluded that this cavity was an enlargement of the right anterior ventricle, effected in all probability by the gradual extenſion of the contained cyſt. I covered the opening with a piece of muſlin, dried the flaps of the ſkin well,

well, replaced and covered them with a piece of linen, and ſecured the whole with an adheſive plaſter.

On removing the fetters the animal roſe without difficulty, and walked to her ſhed without appearing in the leaſt diſpoſed to turn or ramble.

That evening, about eight hours after the operation, ſhe ate a ſmall quantity of hay, and the next morning did not exhibit the ſmalleſt ſymptom of derangement. Having ſome affairs which called me to the continent, I was neceſſitated to confide the animal to other care, and on my return, five months afterwards, was told that ſhe died ſixteen days after the performance of the operation. The account given me was, that ſhe was very well for the firſt five days, when ſhe was dreſſed; that on her appearing dull after the dreſſing, the proprietor conceiving the bandage might be too tight, took it off, and replaced it; that in two hours ſhe appeared to be re-eſtabliſhed, and ate and ruminated as uſual, but was not dreſſed with any regularity afterwards; that on the twelfth day ſhe became heavy, refuſed her food, lay down, and ſhewed ſymptoms of pain and inquietude; and that on the ſixteenth the proprietor, in compaſſion for her ſufferings, ordered her to be killed by opening the veſſels

veſſels of the neck; it muſt, however, be obſerved, that this was done from a perſuaſion that her ſituation did not admit of recovery.

The opening of the head was effected by a blow of an axe, which produced ſuch a confuſion of parts as prevented a ſurgeon who was preſent from diſtinguiſhing any thing more than a very conſiderable quantity of maggots (*pupæ* of the fleſh fly) within the ſubſtance of the brain.

It is to be regretted that a proper attention was not given to this caſe; for, circumſtanced as it was, we cannot draw any concluſion from the practice which was adopted. Perhaps the ſuppuration of the brain would have deſtroyed the animal; but this ſuppuration was certainly increaſed by the admiſſion of air and by other cauſes of irritation.

The capſule or bag was thin, rather opaque, and tolerably ſtrong, without any appearance of vaſcularity; its external ſurface was in general ſmooth; in a few points, however, it was rendered irregular by the adheſion of ſmall, white globular bodies. The internal ſurface was in ſome places perfectly ſmooth, whilſt in others, on the contrary, it was ſtudded with groups of the bodies juſt mentioned, ſome of which were not larger than grains of poppy ſeed and nearly globular;

globular; others, however, were as large as a ſmall pin's head, ſomewhat pyriform, and hung from the cyſt by a kind of neck. In ſome places they were ſcattered at a diſtance from each other, whilſt in others they were accumulated in ſuch numbers as to form cluſters, which hung down into the cavity of the capſule, and bore no ſlight reſemblance to ſmall bunches of grapes. Each of theſe bodies conſiſted of a veſicular worm, or animal hydatid, contained in a ſmall capſule, and which, from the circumſtance of its being found in great numbers in one common capſule, has been called the *ſocial hydatid*, to diſtinguiſh it from another ſpecies, which is generally met with iſolated, and thence named the *hermit* or *ſolitary hydatid*. This hydatid conſiſts of a head, neck, and body, and appears to be of the ſame ſtructure with the larger or ſolitary kind; but as I ſhall have occaſion to ſpeak of theſe worms in another paper, I ſhall reſerve what I have to ſay of their ſtructure and mode of life till that time.

Although I may riſk being cenſured for prolixity, yet I cannot avoid ſtating ſome of the cauſes which concur to render this complaint almoſt always fatal, and invariably highly dangerous.

Theſe

Theſe are,

1. The injury done to the brain by the preſence of ſuch a foreign body.

2. Its difference in ſituation and ſize.

The injury done to the brain may be conſidered as relative to the ſize and ſituation of the capſule.

Its different ſituation produces a degree of uncertainty and difficulty in aſcertaining the part of the cranium neceſſary to be perforated. Its difference in ſize is an object of no ſmall importance, as the degree of ſuppuration which muſt neceſſarily take place after the extraction of the capſule muſt be proportionate to the expoſed ſurface of the brain.

The cyſt is occaſionally to be met with in every part both of the cerebrum and cerebellum. When ſeated ſuperficially in the cerebrum, its preſſure againſt the poſterior ſurface of the part of the bone which is before it excites the action of the abſorbents, which in a certain time remove the correſponding part of the bone, and a ſoft ſpot is left in its place, which ſerves as a guide to the operator *.

When

* There are occaſional deviations from this courſe. In the caſe before us the efforts of nature appear to have been

ill

When ſeated towards the baſis of the cerebrum, it meets with greater reſiſtance than when on the ſurface, increaſes in ſize, and produces a proportional diminution in the volume of the brain before it can effect the ſoftening of the correſponding part of the bone.

As I have not met with it in the cerebellum myſelf, I cannot ſay whether it is able to produce the ſoftening of the poſterior or any other part of the cranium, or whether the animal in which it is found is not deſtroyed before this effect takes place.

Its ſize may be conſidered as relative to its age and depth; for if it be deep ſeated, it muſt, by its extenſion, diſplace much brain before it can induce the ſoft ſpot; whilſt, on the contrary, if ſuperficial, the ſoft ſpot may take place without any very conſiderable extenſion of the capſule.

From what has been ſaid, it muſt appear that the moſt favourable caſes for the performance of the operation are thoſe in which the ſoft ſpot takes place ſoon after the appearance of the

ill directed by ſtrengthening the dura mater inſtead of weakening it, and the bone had not undergone any perceptible change.

ſymptoms,

ſymptoms, in conſequence of the capſule being ſuperficial; but even here it muſt be undertaken with a very guarded prognoſtic.

The moſt unfavourable caſes are thoſe where either the ſoft ſpot does not make its appearance at all, or where it takes place after the other ſymptoms have been preſent for a conſiderable time, and where great debility and emaciation exiſt.

Half-moon Street,
Piccadilly,
March 30, 1792.

V. *Facts relative to the Prevention of Hydrophobia. Communicated in a Letter to Dr.* Simmons *by Mr.* Jeſſe Foot, *Surgeon in London.*

I HAVE the honour of preſenting to you ſome facts relative to the prevention of hydrophobia, which I wiſh to ſee publiſhed as an excitement to a ſimilar mode of treatment in other inſtances of the ſame kind; being thoroughly perſuaded that exciſion of the bitten part is the only remedy that ought to be truſted to in ſuch caſes.

The facts now communicated to you are in addition to two other ſtriking and ſucceſsful inſtances of the ſame ſort formerly given to the Public, by me, in an Eſſay * on this ſubject, to which I beg leave to refer you.

CASE I.

Two years and a half ſince a young lady was bitten in the heel by her own lap dog, ſo as to produce a bleeding; the dog died mad the next day. I was preſent when the bitten parts were cut out, and the lady has remained well. The operation was performed three days after the accident.

CASE II.

Sixteen months ago a ſtrange dog came into the yard of Robert Jackſon, Eſq., near Tewkſbury in Glouceſterſhire, and bit two dogs: they were both bathed in the ſea, and to both the Ormſkirk medicine was given. They both died mad; but one of them, having got looſe during his madneſs, bit a puppy that he had formerly been very fond of: the bitten part of the

* An Eſſay on the Bite of a Mad Dog. 8vo. London, 1788.

puppy

puppy was cut out directly, and the puppy continues well.

CASES III. AND IV.

For these two cases I am indebted to Mr. John Capon Weeks, Surgeon at Rochester, and I shall give them to you in his own words, extracted from his letter to me on this subject, dated March 12th, 1791.

" George Cobb, a servant of Commodore " Pasley, was bit by a dog on the 29th of " November, 1789. I saw him immediately " after the bite. The bite was on the right " cheek, and the inside of the upper lip; but " I could not persuade him to have the parts " that were bitten extirpated. The Ormskirk " medicine was made use of, and the Birling " medicine was also given. The lad continued " very well for ten days after the bite, and the " wounds healed; but on the 10th of Decem- " ber, symptoms of hydrophobia appeared. " The Birling medicine was now again admi- " nistered; but by two o'clock of the morning " of the 12th of December the patient ap- " peared quite exhausted by the violence of " the symptoms, became quiet, and died in " about half an hour after.

" Richard Braham was bit by the ſame dog " in about a quarter of an hour after the bite " of the unfortunate George Cobb. I ſaw this " lad about an hour after the bite. The parts " bitten were the upper lip, the under part of " the lower jaw, and the little finger of the " right hand, all of which bled in conſequence " of the bites. The bitten parts were all ex- " tirpated. The wounds were dreſſed with a " mercurial digeſtive, and healed in about ſix- " teen days. This patient has experienced no " ill effects from the bite, and is now in good " health."

Dean Street, Soho,
April 7, 1792.

VI. *Two Caſes of Fracture; one of the upper, the other of the lower Jaw.* *By Mr.* T. Hughes, *Surgeon at Stroud-Water in Glouceſterſhire.*

CASE I.

ON the 22d of April, 1768, a lad, about ſixteen years of age, received ſo violent a kick in the face from a horſe as to throw him backwards.

backwards. The accident happened a mile or two from his maſter's houſe: he got up and walked home, bleeding all the way. I ſaw him a few hours after the accident, and found the upper jaw fractured. The fracture extended horizontally from the poſterior part of the right os maxillare, beyond the fartheſt tooth, as far as the cuſpidatus * on the left ſide, juſt above the alveolar proceſs, and nearly where the cheek and lip are connected to the gums. The inciſor teeth were beaten out, with the anterior part of their ſockets; and the poſterior part of the ſockets was detached from the other part of the fractured piece. The molares, bicuſpides, and cuſpidatus, of the right ſide, remained firm in their ſockets. The fracture extended alſo in an irregular manner through the bony palate, and the membranes were lacerated, ſo that the air ruſhed through the opening from the noſe into the mouth. By this extent of fracture all the alveolar proceſs of the right os maxillare, with its molares, bicuſpides, and cuſpidatus teeth, that part of the pterygoid proceſs of the os ſphenoides connected with it, and as much

* According to Mr. Hunter's arrangement in his Natural Hiſtory of the Human Teeth.

of the left os maxillare as form the ſockets of its two inciſors, with part of the bony palate, were beaten into the mouth, and made a very frightful appearance. He was very ſenſible, and ſpoke, but could hardly be underſtood. The fractured piece was eaſily reduced, but fell down immediately upon the fingers being removed, being held by the ſoft parts only.

I felt myſelf at a great loſs how to ſupport the fracture, from the want of ſome fixed point at the back part of the mouth, from not being provided with implements, and from its being late in the evening, in a country place, five miles diſtant from Wotton-Underedge, my then place of reſidence. I thruſt ſome compreſſes between the teeth of the upper and lower jaw of the affected ſide, and paſſing a ligature through a convenient part of the bone near to the left cuſpidatus, tied it to that tooth. By theſe means the piece was conſiderably, though not ſufficiently, raiſed, eſpecially the palatine part. The patient was put to bed, ordered to be kept cool and quiet, and to be fed with a liquid, laxative diet through the ſpout of a tea-pot.

I found the next day, that, the compreſſes and ligature having ſlipped, the fractured piece was fallen down; but an eryſipelatous tumour of

of the face, attended with fever, having come on, nothing could be done in that ſtate to redreſs the fracture. Some blood was taken away, and a laxative mixture directed.

Two days afterwards Mr. Cooper, my partner, ſaw him with me. The eryſipelas being nearly ſubſided, we deliberated on the ſteps to be taken. While my thoughts were employed how to get over the difficulty of making a perforation through the firm part of the os maxillare, by which a ligature might be paſſed, and the fractured piece ſecured, Mr. Cooper, who ſeemed to be meditating ſome kind of prop, having raiſed the fractured piece with his finger, thruſt one end of the ſponge, with which we had been cleaning the parts, into the patient's mouth. We both immediately ſaw with great pleaſure that this ſubſtance, by its expanſibility, would effectually ſupport the parts in their proper ſituation. A piece, therefore, of a proper ſhape and ſize, ſo as moderately to fill the mouth, leaving ſufficient room on the left ſide for the ſpout of a tea-pot to be introduced for the conveyance of nouriſhment, and with a ſtring faſtened to it, was introduced. By this ſimple, eaſy method the fractured piece was ſo well ſupported in its proper ſituation, that,

upon changing the ſponge thirty-ſix hours afterwards, the roof of the mouth was raiſed to its natural arch, which before could not be ſo well effected even with the finger.

The diſcharge of matter and mucus was ſo large and offenſive as to make it neceſſary afterwards to change the ſponge daily; and for this purpoſe, though the patient was much reduced, he received ſufficient ſupport from liquid nouriſhment to enable him to walk to Wotton from the village where he lived. After the firſt application of the ſponge, it was uſed in a dry ſtate, that it might abſorb more of the diſcharge. In about a fortnight the diſcharge leſſened, and the parts acquired a firmneſs ſufficient to enable him to change the ſponge himſelf, and in about a month there appeared to be no farther occaſion for it. The remaining poſterior part of the ſockets of the fore teeth were removed as they became looſe; as alſo a ſmall exfoliation from the palatine proceſs of the maxillary bone. The ſoft parts healed, and left no fiſſure in the palate.

By the uſe of the ſponge the fractured ſide of the upper jaw was ſo much raiſed, that the molares and bicuſpides teeth projected a trifle beyond their natural ſituation; but ſome time after

after it had been laid aſide I obſerved that the teeth had fallen a little, ſo as to ſtand rather within the outer edge of the correſponding teeth of the lower jaw when the mouth was ſhut. From the ſame cauſe there was a ſlight depreſſion of that ſide of the face below the cheek bone. I obſerved alſo, that, from the violence committed by the accident, the left naſal bone, the lower edge of the orbit of the left eye, and the naſal proceſs of the left maxillary bone, were ſlightly raiſed: but from all this he could not be ſaid to have any deformity or defect, except what aroſe from the loſs of the fore teeth.

REMARK.

I do not know that any improvement could have been made in the uſe of the ſponge in this caſe, except that, inſtead of uſing it dry, it had been always ſqueezed out of water, or any other agreeable liquid, and changed twice or oftener in twenty-four hours, which this patient's ſituation did not admit of.

CASE II.

In the evening of May 16, 1780, a boy, ſix years old, had his lower jaw fractured by a horſe,

horſe, which was rode over him, and he was brought to me the next day. The fracture was nearly perpendicularly acroſs the jaw, betwixt the ſecond inciſor and cuſpidatus teeth on the right ſide, and from the poſterior part of the cuſpidatus extended obliquely, behind the inciſores and left cuſpidatus, through the alveolar proceſs and baſe of the jaw to the anterior part of the firſt left molaris, in ſuch a manner, that the anterior part of that portion of the jaw forming the chin and incloſing the four inciſores and left cuſpidatus teeth was ſevered from the poſterior part, ſo as to leave a large chaſm between them, the gums being alſo torn, and forced downwards and to the left ſide, ſo as to be below its natural level from half or three quarters of an inch on the right ſide to three quarters of an inch, or an inch, on the left ſide, and to be about a finger's breadth diſtant from the correſponding right ſide of the jaw, and of courſe overlapping the left ſide as much; the teeth in the detached piece remaining fixed in their ſockets. The parts bled freely at the time of the accident, and the chaſm between the fractured pieces was now filled with grumous blood.

It was not very difficult to bring the fractured pieces to correſpond on the right ſide; but I

could not raiſe the depreſſed piece ſufficiently on the left to correſpond with that ſide of the jaw, owing partly, as it ſeemed, to the points of the fracture entangling each other, and partly to the action of the muſcles upon the boy's crying and endeavouring to ſhut his mouth in making the neceſſary attempts. No uſe could be made of ligatures to the teeth; and the uſual bandage was of no ſervice in ſupporting the depreſſed piece; however I left it on to keep the parts a little ſteady, and ordered that nouriſhment ſhould not be given by the mouth, but by clyſters, till I contrived and got made an apparatus more adequate to the purpoſe. This conſiſted of a couple of little irons, which acted on the principle of what mechanics call holdfaſts, to reſt on the teeth in the ſides of the jaw above, and fixed below and before, by means of ſcrews, to a caſe of copper, adapted to the form of the jaw, and ſecured by tapes faſtened to a cap on the head. This, properly defended, was applied on the 20th; at which time, beſides a conſtant flow of ſaliva, mucus, &c., from the mouth, there was conſiderable ſwelling and hardneſs under the jaw; and the leaſt attempt to hold back the head put him to great pain. Directions were given to keep the parts clean by frequently

frequently injecting barley water with honey of roſes and tincture of myrrh.

By means of the machinery, the depreſſed piece of jaw was at firſt kept in a much better ſituation, and the parts ſo ſteady, that the boy could move his head much more eaſily than before, and take liquids; and I hoped to derive great advantage from it. But although alterations were occaſionally made in its form and application, I afterwards found that, from the difficulty of keeping the left holdfaſt far enough back, and of preventing one or other from ſlipping now and then, and from the lip ſwelling from the neceſſary preſſure of the irons, circumſtances which appeared to irritate and render him reſtleſs, and as a greater force applied with a view to ſupport the depreſſed piece only increaſed the ſeparation, by exciting the action of the muſcles of the jaw, and as I ſaw that exfoliations would take place in the alveolar proceſs; on all theſe accounts I laid aſide the holdfaſts, after uſing them four days, and continued the uſe of the copper caſe only.

I frequently thought of the advantage received from the ſponge, in the caſe of the fracture of the upper jaw above related; but did not at firſt conceive that, in the preſent caſe, any advantage

vantage could be reaped from it. However, after ſome days, it occurred to me, that a piece of dry ſponge, made flat by preſſure, and placed on the inſide of the copper caſe, by ſwelling from the conſtant flow of ſaliva, &c. from the mouth, might gradually raiſe the depreſſed piece of jaw, without occaſioning pain. This was applied on the 30th, a fortnight from the time of the accident. Having found nothing applicable to the caſe in authors, I related it to ſome gentlemen of my acquaintance of experience and abilities, who readily informed me of what they had done in ſomewhat ſimilar caſes, but which could not be uſed in the then ſtate of my patient, except wedges of cork placed between the teeth on the ſides of the jaws; I, therefore, tried them, with the ſponge under the chin at the ſame time. The wedges were uſed three days, cut in various forms, as ſeemed beſt to keep the fractured parts ſteady, and themſelves from ſlipping; but, I was obliged to give them up, as they occaſioned great pain, and either brought on or increaſed an hectic fever, attended with diarrhœa, by which the patient was ſo much reduced that his life was not expected. The uneaſineſs he ſuffered from the wedges ſeemed to ariſe, partly from their ſlipping ſometimes

and thereby ſeparating the fractured pieces, but chiefly from their keeping the jaw in a depreſſed and unnatural ſtate. The left cuſpidatus tooth, by bearing againſt and overlapping the adjoining molaris, ſeemed to be another obſtacle to the raiſing of the depreſſed piece: I, therefore, removed the cuſpidatus, and then truſted to the power of the ſponge, applying a dry piece daily betwixt the chin and copper caſe, and occaſionally alſo keeping another bit of ſponge betwixt the right cheek and ſide of the jaw, (which was carried too much outwards), and continuing to waſh the parts with the injection. When this method was firſt uſed, the middle detached piece of the jaw was ſo much below its natural level, as eaſily to admit nouriſhment to be conveyed between the upper and lower teeth, by means of a tea pot with a flat ſpout, or a ſpoon.

In a few days he became quite eaſy, and his general health mended; alſo, the depreſſed piece of jaw was gradually raiſed, ſo that by the end of June the fore teeth of the lower approached thoſe of the upper ſo nearly, that he retained moſt of the ſaliva; and as it advanced, the fracture acquired firmneſs likewiſe to admit of his opening his mouth with eaſe, and receiving ſome ſolid food, brought to a ſtate not requiring maſtication.

maſtication. By the middle of July, there was neither waſte of ſaliva, nor any further occaſion for the ſponge. The copper caſe was uſed conſtantly a month longer; and afterwards, when he was eating, till the beginning of September. He began to maſticate ſolid food in the middle of Auguſt. From the middle of June to the middle of Auguſt, ſeveral pieces of the alveolar proceſs exfoliated; alſo a ſmall bit of the jaw was caſt off on the left ſide, through a ſmall opening it had produced in the teguments; and a tooth fell out, which proved to be the body of a cuſpidatus of the ſecond ſet, its fang not yet formed. During the cure, no advantage was received from internal medicine, as he could not be prevailed with to take any.

In September, the appearances of the parts concerned were theſe: When the mouth was ſhut, no mark of diſeaſe was diſcovered, except a ſlight pit in the teguments under the left ſide of the jaw, where the external exfoliation happened. On turning down the under lip, the teeth and gums were perfectly regular in the circular range; though the left cuſpidatus was deficient, there was no chaſm betwixt the inciſor and molaris; and there was ſo little diſtance betwixt the upper and lower inciſors, that it would

would not have been obſerved unleſs pointed out. Indeed, the greateſt deformity was what aroſe from the fore teeth of the lower jaw being incruſted with tartar, from want of uſe.

From October to December he ſhed the milk inciſors; and, the firſt molaris on the left ſide having been looſened by an exfoliation, I removed it, and at the ſame time the crown of a bicuſpis of the ſecond ſet.

In June 1787, he then being thirteen years old, I had an opportunity of examining him. The right cuſpidatus tooth, and one of the bicuſpides on the left ſide were deficient, as could not otherwiſe be expected; the reſt of the teeth were very regular, more ſo than the teeth of perſons in general who have the full number. There was a ſmall chaſm, but not very ſtriking, betwixt the ſecond inciſor and the firſt bicuſpis on the right ſide. Though all his teeth were large, the bicuſpides on the right ſide, and the bicuſpis and cuſpidatus on the left, ſeemed to be larger than uſual. The teeth of the lower not only met thoſe of the upper jaw, but by carrying the jaw backwards, he could bring the lower behind the upper ones.

REMARKS.

It is ſeen from the above account, that, in the reduction of the fracture, what could not be done at once by force, was brought about gradually and with eaſe; which mode of conduct may be advantageouſly purſued in other fractures, when difficulties oppoſe a ſpeedy reduction.

It appears alſo, that there was an imperfection in the cure, by the line of the jaw being ſhortened, and conſequently not leaving ſufficient room for the full number of teeth; but perhaps the cure was as perfect as the circumſtances of the caſe would admit. And, indeed, unleſs thoſe of the ſecond ſet of teeth, which were loſt, could have been ſaved; it was an advantage, in the event, to have the circle of jaw leſſened likewiſe, otherwiſe, there muſt have been chaſms between the teeth, which nature could not have filled up. On the other hand, had all the teeth been preſerved, unleſs the circle of the jaw could have been extended by keeping its ſides at a greater diſtance, the teeth muſt have ſtood in a very irregular manner.

Stroud Water,
May 8, 1792.

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VII. *Case of an enlarged Nympha. By Mr.* William Morlen, *Surgeon in London.*

THE subject of this case was a young woman, who, at the age of sixteen years, married, and contracted the venereal disease from her husband. A bubo formed, which suppurated; and about four months from the commencement of the disease she perceived a considerable swelling in the entrance of the vagina, of the size of a walnut, which gave her no inconvenience except in the embraces of her husband. The bubo being at this time healed, and no farther remains of the venereal disease (as she thought) seeming to exist, she neglected to consult any one about this swelling for nearly two years, by which time it had attained the size of a man's fist. It now beginning to alarm her, she applied to an apothecary in her neighbourhood, who, after examining it in a cursory manner, pronounced it a *procidentia uteri*, and recommended some internal medicines, but made no attempt, so far as I can learn, to reduce it.

The patient, finding no relief from a long continuance of this treatment, by the advice of a midwife

a midwife applied to me, and, upon examination, I found a moveable tumour, of the ſize before mentioned, projecting from the labia pudendi the diſtance of four inches. Its preſſure on the lymphatics had enlarged the labia pudendi to an enormous degree, and the irritation which it produced in walking, or even ſitting, occaſioned an increaſed ſecretion, or fluor albus, which had excoriated the adjoining parts, ſo as to form a conſiderable ulcer *in perinæo*.

Such was the appearance of things externally. Upon introducing my finger into the vagina, I clearly aſcertained the uterus in its natural ſituation, and unimpregnated, which removed every doubt of the tumour's being that organ; and upon minutely examining the ſituation and attachment of the tumour, (with the aſſiſtance of Dr. Clarke, of Queen Street, Golden Square) it was aſcertained to be an enlarged nympha of the right ſide. As the removal of the tumour by the knife ſeemed to be the only means by which relief could be obtained, we propoſed it to our patient; and having obtained her aſſent to the operation, I performed it on the 30th of March, aſſiſted by Mr. Harris, Surgeon.

The tumour (which is now in the poſſeſſion of Dr. Clarke) was found, when removed, to weigh ſeven ounces and one drachm. The hæmorrhage being conſiderable, we were under the neceſſity of having recourſe to the ligature. After the removal of the tumour the ſwelling of the labia pudendi gradually gave way to mercurial friction, and the ulcer *in perinæo* ſoon healed.

Any coheſion of the wounded parts, which might perhaps otherwiſe have taken place, was prevented by the introduction, into the vagina, of a piece of wax candle, which was continued there till the inflammation of the parts had ſubſided.

Harpur Street,
May 10th, 1792.

VIII. *An Account of the good Effects of Electricity in a Caſe of violent ſpaſmodic Affection. By Mr.* George Wilkinſon, *Surgeon at Sunderland, and Member of the Royal College of Surgeons at Edinburgh, &c.*

ON the 25th of May, 1788, I was deſired to viſit Miſs A. Crawford, of this place, aged

aged twenty-eight years, and of a delicate conſtitution, who had for about three weeks been afflicted with a violent ſpaſmodic complaint. I found her labouring under great dejection of ſpirits; her pulſe was weak, but regular; her appetite was much impaired, yet ſhe had little or no ſickneſs or thirſt: ſhe complained of a ſenſe of weight in her forehead, with dimneſs of ſight, and of a pain about the region of her ſtomach; ſhe was coſtive; her urine, which was much increaſed in quantity, was ſometimes limpid, but in general of an amber colour, and ſhe frequently complained of coldneſs, particularly in her extremities.

On farther inquiry, it appeared that ſhe had enjoyed a tolerable ſhare of health till the 7th of May, when, after much uneaſineſs of mind, ſhe was ſeized at two o'clock in the morning, while in bed, with a rigidity and ſtiffneſs of the whole body. At four ſhe became totally inſenſible, and continued ſo till eight the ſame morning, when, after ſeveral deep ſighs, the fit went off, her limbs became relaxed, and her ſpeech and recollection gradually returned.

She had no more attacks of this kind till about a week afterwards; and then they differed greatly in their violence and duration, reſem-

bling hyſteric affections, frequently coming on twice or thrice in the ſpace of an hour, and continuing only a few minutes at a time. It often happened that they left her for the ſpace of a day, and ſometimes for two or three days, but when this was the caſe they returned with greater ſeverity, more eſpecially at the approach of the catamenia, (which were always regular) and previouſly to the coming on of rain or ſtormy weather, obſerving no regular periods with reſpect to their attack, and varying in their duration from fifteen minutes to two hours or longer.

There was no criterion whereby ſhe could aſcertain the approach of theſe fits, their acceſſion being always ſudden. An univerſal ſpaſm, producing a rigidity of the whole frame, took place in a moment, and deprived her of the power of ſpeech and recollection. Indeed the frequent opportunities I had of ſeeing her in theſe ſituations enabled me to obſerve the progreſs of the ſymptoms with attention, and her caſe ſtruck me as bearing a ſtriking reſemblance, in many reſpects, to that of a young lady deſcribed by a late ingenious and reſpectable writer.

" She

" She exhibited," ſays he, " a figure of " death-like ſleep, beyond the power of art to " imitate, or the imagination to conceive. Her " forehead was ſerene, her features perfectly " compoſed. The paleneſs of her colour, her " breathing at a diſtance being alſo ſcarce per- " ceptible, operated in rendering the ſimilitude " to marble more exact and ſtriking. The " poſition of her fingers, hands, and arms, " was altered with difficulty; but they preſerved " every form of flexure they acquired *."

Previouſly to my attendance ſhe had taken ſeveral medicines, ſuch as valerian, and the fœtid gums in various forms; from theſe, however, ſhe had experienced little or no benefit.

The intention I had in view was to obviate the attack of the fits, by diminiſhing the irritability of the ſyſtem, and to reſtore the ſtomach to its natural functions; for this purpoſe I recommended that, on the acceſſion of the fits, her hands and feet ſhould be immerſed in warm water, and a draught compoſed of vin. antim. æther vit. and tinct. opii, in a ſuitable vehicle, to be taken at proper intervals, varying the doſe

* *Vide* Select Caſes by John Jebb, M. D. 8vo. London, 1782, p. 64.

of the ſeveral ingredients according to the urgency of the ſymptoms.

By this treatment the ſtrength and frequency of the fits were diminiſhed.

In the intervals, or abſence of the fits, ſhe took, at proper intervals, as tonics, extract of gentian and rhubarb, with ſalt of ſteel. From the uſe of theſe remedies ſhe experienced great benefit: her appetite improved, and the fits left her for a week.

Bark in ſubſtance, to the quantity of two ſcruples three times a day, being afterwards adminiſtered, diſagreed with her ſtomach, and ran off by the inteſtines, though combined with cinnamon in powder, and with ſmall doſes of tincture of opium.

June 7th, her fits again returned, and continued to attack her more frequently, and with increaſed violence, till the 13th, when I was ſent for in great haſte, and found her ſitting in her chair, perfectly ſenſible, but with her jaw completely locked, her face ſomewhat diſtorted, her head drawn backwards, and the muſcles of her neck rigid and inflexible. The approach of this fit had been preceded, as ſhe informed me, by a ſenſation like the cramp in the ſoles of her feet, ſucceeded by a coldneſs of the extremities,

and a ſpaſmodic affection of the muſcles of her neck and jaw.

Under theſe circumſtances, her hands and feet were immerſed in warm water; her neck and throat were repeatedly rubbed with a liniment compoſed of volatile alkali, mixed with ol. terebinth.; a fœtid clyſter, with the addition of a drachm and a half of tincture of opium, was likewiſe adminiſtered; and from thirty to forty drops of the latter, mixed with an equal quantity of æther, were directed to be given every two or three hours in a ſuitable vehicle.

This plan of treatment was purſued till the next morning, when, upon finding her no better, I determined to try the effects of electricity.

Mr. Martin, of this place, who has applied himſelf, with great ſucceſs, to medical electricity, having favoured me with his aſſiſtance, we placed the patient at firſt in an inſulated chair connected with a pretty powerful machine, the cylinder of which was eleven inches in diameter; ſtrong ſparks were taken from various parts of her body, but particularly from the muſcles of the neck: theſe proving ineffectual, ſeveral ſmart ſhocks, by means of directors, were given in the ſame way, and acroſs the jaw, near

near the articulation. Theſe alſo proving unſucceſsful, the ſhocks were next applied through the whole courſe of the ſpine. When ſhe had received about half a dozen ſhocks, in this way, a profuſe perſpiration took place, and in the ſpace of eight or ten minutes her mouth, which had been cloſed eighteen hours, opened almoſt inſtantaneouſly, and ſhe regained her ſpeech.—For ſome days after this there was a diſpoſition to tetanus ſtill prevailing, though the jaw did not again become completely locked; electricity was therefore occaſionally had recourſe to for the ſpace of a week; tonic remedies (but without the bark) were likewiſe adminiſtered, and a plaſter compoſed of empl. ſtomach. opium, and camphor, was applied to the region of the ſtomach. Under this mode of treatment the fits gradually went off, and ſhe regained, at the end of about ſix weeks from the time I firſt attended her, a perfect ſtate of health.

A period of near two years having elapſed, ſhe was again ſeized, on the 13th of June, 1790, with a fit exactly ſimilar to that which had taken place on the 7th of May, 1788, and the day following her jaw became rigid, and completely locked. As I was from home at this time, my friend Mr. Martin again gave her his aſſiſtance.

The

The account he gave me of her ſituation was, that he found her in a ſtate of ſtupor, with her countenance diſtorted, her head drawn backwards, and her whole frame ſtrongly convulſed.

Being unable to get his large cylindrical machine, he was obliged to employ a ſmall globe one, and a Leyden phial containing about a ſquare foot of coating; with theſe he paſſed ſeveral of the ſtrongeſt ſhocks he could well collect from the phial through her legs and arms, and in the direction of the ſpine, for near an hour, by which means the contraction of her limbs went off, and ſhe recovered her ſenſes; but the ſpaſm of the jaw did not ſubſide till about half a dozen ſhocks had been ſent through it by means of a director fixed under each ear. She was then put to bed, but before ten minutes had elapſed her jaw again became locked, and was again relieved by the application of three more ſhocks, paſſed in the ſame direction as the former ones. In this manner the tetanus continued to return and to be relieved for ſeveral hours, till at length the ſlighteſt ſhock was found ſufficient to produce the effect of opening her mouth.

My abſence from home prevented me from ſeeing her till two days afterwards. I then

found that the electricity had been occaſionally repeated, on the recurrence of the fits, which had now almoſt totally ſubſided. Tonic remedies were adminiſtered, and the patient remained free from any ſpaſmodic complaint for a fortnight. At the end of that time, in conſequence of fatigue, ſhe had a return of the fits, and her jaw again became locked, and remained ſo for ſeveral hours, till the ſame remedy, the good effects of which ſhe had ſo repeatedly experienced, was again had recourſe to. Strong ſhocks were paſſed acroſs the jaw and down the ſpine, as before, and by theſe means, in about twenty minutes, the ſpaſm was removed. After this laſt attack ſhe gradually recovered, and has remained well ever ſince.

This caſe ſeems to be worthy of being recorded, both on account of the uncommon ſymptoms that attended it, and of the relief obtained in it from electricity.

M. Sauvages, in his *Noſologia Methodica*, has arranged, among his ſpecies, a *catalepſis hyſterica* and a *tetanus hyſtericus*; but of neither of theſe has he been able to collect more than a ſingle inſtance; ſo that they muſt be conſidered as rare affections; and their combination, as in the

the caſe I have related, is probably a ſtill more uncommon occurrence.

M. Lieutaud * ſuppoſes that what has been called catalepſy is conſtantly an hypochondriacal or hyſterical affection. That it was ſo in the caſe of my patient I am very ready to allow; but how far his opinion is applicable to other caſes of catalepſy, as deſcribed by different writers, I ſhall not at preſent attempt to determine.

IX. *Caſe of a ſingular cutaneous Affection; with ſome Remarks relative to the Poiſon of Copper. By Mr.* William Davidſon, *Apothecary in London. Communicated in a Letter to Dr.* Seguin Henry Jackſon, *Phyſician in London, and by him to Dr.* Simmons.

ON the 16th day of December, 1787, I was ſent for to a family in my neighbourhood, where the mother and four children were affected with a cutaneous eruption, which had made its appearance that day, and occaſioned them much

* Precis de la Medecine pratique, Tome I. p. 302, 8vo. Paris, 1777.

uneaſineſs.

uneaſineſs. This eruption was ſeemingly of the leprous kind, and conſiſted of ſpots of different ſizes, the largeſt of which were white and ſcaly, with moiſt baſes, appearing as if ſomething acrimonious had been ſecreted under the cuticle, which thickened, raiſed, and ſeparated it from the cutis. It was more or leſs all over the body, and very much amongſt the hair of the head. There was no itching, or particular pain; nor did the general health of the patients ſeem affected.

Knowing that affections of the ſkin often ariſe from thoſe of the ſtomach, I queſtioned them about their diet, and particularly inquired whether they had taken any thing that day which had diſagreed with them. I was informed, that, two days before, the above five perſons and three young women (viſitors) had all dined upon peaſe ſoup, which was diſtributed with a braſs ladle that had been in the warm ſoup ſome little time; that the ladle, having been long out of uſe, and hung up expoſed to the open air, was quite green, but which was not noticed at the time; and that the mother, having been much alarmed, and imputing the appearances on the ſkin to this circumſtance, had ſent to the three viſitors, and, finding they were all affected

in

in the ſame manner, concluded they were poiſoned, unleſs I could afford them ſome immediate relief.

The mother was about twenty-ſix years of age, and in the ſecond month of her pregnancy, although ſhe was ſtill ſuckling the youngeſt of the patients, a girl then about fourteen months old. The ſpots upon this child were not very numerous; and it was obſerved that ſhe had taken only a ſmall quantity of the ſoup. The eldeſt daughter was between eleven and twelve years of age; the next, about nine; and the other child, about four. The young women were about ſeventeen or eighteen. None of them had taken any thing which had occaſioned ſickneſs or any internal affection; and even the peaſe ſoup, which certainly contained more or leſs of verdigris, had occaſioned no diſturbance in the ſtomach, and was digeſted as uſual.

I found, on examination, that the quantity of eruption was in an exact proportion to the quantity of ſoup taken by each perſon; for the mother, who wiſhed to avoid peaſe ſoup, thinking it improper for her who was then giving ſuck, and only took a ſmall quantity, had leſs of the eruption than any of the reſt; while one of the girls, who had eaten plentifully of it, was almoſt

moſt covered with this leprous appearance. From this laſt circumſtance, and from the length of time which intervened between the taking of the ſoup and the appearance of the ſpots upon the ſkin, I was led to believe that this eruption could not originate from that ſympathy between the ſkin and ſtomach to which may be aſcribed ſo many of the cutaneous affections, but ought rather to be imputed to the copper mixed with the ſoup, and which, having been taken into the ſtomach, abſorbed, and carried into the blood veſſels, the conſtitution was now endeavouring to diſcharge by throwing it upon the ſurface of the body. I, therefore, conceived that the cure would chiefly depend upon encouraging theſe efforts, and at the ſame time, if poſſible, deſtroying the virulence of whatever portion of the copper might remain in the blood veſſels.

Having theſe conſiderations in view, and recollecting the well-known effects of ſulphur, not only as a diaphoretic, but alſo in rendering the moſt active metallic ſubſtances mild and innocent, I conſidered it as the beſt medicine I could employ. Accordingly I adminiſtered the *lac ſulphuris* night and morning, in doſes ſo regulated that its chief action might not be exerted

erted in the ſtomach and bowels; fearing leſt, by an increaſed proportion, the periſtaltic motion might be ſo much excited as to check the determination to the ſurface of the body.

Had any urgent ſymptoms been preſent, I ſhould have given it every four or ſix hours. In all the patients the body continued ſufficiently open, and there was more or leſs of gentle perſpiration in the night. For the firſt five days the ſpots kept coming out; but from that period they became browner, and at laſt gradually diſappeared, without leaving any mark behind. The medicine was continued only about ſeven or eight days, as the patients felt themſelves in perfect health, and the eruption ſeemed going off. Indeed the whole ſoon diſappeared, excepting on the girl who had eaten moſt heartily of the ſoup. Upon her it continued for near two months. The family remained afterwards in perfect health, and have never had any return of this or any ſimilar complaint; nor were they ever before affected with any diſeaſe of this kind.

I ſhall juſt beg leave to obſerve, that the ſoup was made of freſh beef, and in a tin ſaucepan; ſo that, excepting the verdigris on the ladle, there was no other ſubſtance which could produce

any of the above-mentioned appearances*. Here the copper was mixed in a vehicle the beſt adapted for leſſening its ſtimulating powers, for blunting its emetic property, and allowing it inſidiouſly to get into the blood. Whether it there retained its original form of aërated or acetated copper, and was in that ſtate effuſed under the cuticle by the exhalent arteries, ſome future experiments may, perhaps, determine. It is well known that copper, in this ſtate, will diſſolve in a watery menſtruum; for a conſiderable portion of verdigris will diſſolve in cold diſtilled water, making a green ſolution.

If then my inference be allowed, that the copper, a very irritating, poiſonous ſubſtance, was ſo ſheathed as to be introduced into the ſyſtem without producing any particular effects in the *primæ viæ*, might not ſome of our more active medicines be in like manner conveyed into the blood veſſels, ſo as to excite actions in the more remote parts of our ſanguiferous and glandular ſyſtems, which might tend to the cure of the more obſtinate diſeaſes? Every practitioner knows that mercury may be ſpeedily

* Is it neceſſary for me to inform the chemical reader that the ſoup was ſeaſoned with common ſalt?

intro-

introduced into the blood in this way, without paſſing off by the bowels. But at ſome future period chemiſtry may, perhaps, furniſh us with ſome medicines ſtill more active and effectual, which, introduced in this way, may produce ſpecific actions tending to the reſtoration of health.

It may perhaps be worthy of inquiry, whether verdigris produced from pure copper, or from that metal when in the form of braſs, produces ſimilar effects in the human body? A circumſtance having lately occurred which has ſome relation to this ſubject, I ſhall beg leave to mention it:—About four or five weeks ago a fine little boy was brought to me, having upon him a leprous eruption, which appeared ſimilar to the above. He was about three years of age, of a florid complexion, lively, and ſeemingly in good health. The eruption had been out about ſeven or eight days. Having in remembrance the caſe juſt now related, I enquired whether the boy had been in the habit of putting any thing made of copper or braſs into his mouth? His mother told me that he did ſo very often; that he uſed frequently to put halfpence and a braſs thimble into his mouth, which (ſhe ſaid) occaſioned neither ſoreneſs in the mouth, nor ſick-

neſs of ſtomach. From this account I imagined that the eruption might ariſe from the ſame cauſe as in the preceding caſes, (viz. from copper, abſorbed, carried into the blood, and thrown out upon the ſkin) and therefore determined to try the ſame remedy. I accordingly ordered eight powders for him, each conſiſting of ten grains of the *lac ſulphuris*; one of theſe was directed to be taken night and morning. His mother was deſired to bring him to me again in four or five days; but as ſhe omitted to do this, I am unable to give the event of the caſe.

Queen Anne Street, Eaſt.
July 13th, 1792.

X. *Two Caſes of pulmonary Hæmorrhage, ſpeedily and ſucceſsfully cured by Abſtinence from Liquids. By the Same.*

ON the 6th day of March, 1792, I was requeſted to viſit Mr. S——, a man of a florid complexion, full habit of body, and about forty-five years of age. He had been much affected with head-ach, and hard dry cough, for three or four weeks. His pulſe was now full, frequent,

frequent, hard, and quick; and the veins upon his hands and arms were ſo much diſtended, that they appeared as if ready to burſt. The cough was almoſt inceſſant, attended with conſiderable expectoration of florid frothy blood, which made its appearance this morning, after a ſevere fit of coughing, and his head-ach ſtill continued.

The plan I followed was the following:

I took, from a large orifice in the arm, twelve ounces of blood, which from the long time it remained fluid after being taken from the arm, and the conſequent appearance of (what is commonly called) inflammation, both indicating the great action of the ſanguiferous ſyſtem, ſeemed to point out the neceſſity of employing the moſt vigorous antiphlogiſtic treatment. Much danger was alſo to be apprehended from the enlargement of the opening of the ruptured veſſel.

Accordingly, I ordered him a ſaline draught, with antimonial wine, to be taken every three hours, adding to the night draught ſome ſyrup of white poppies, and an opening ſaline draught to be taken the morning following, and repeated every other morning.

This courſe of medicine, together with abſtinence from animal food, and a ſtrict adherence to a light cooling diet, was regularly purſued for

three days; during which time the bleeding, although moderated, ſtill continued, but the cough was much better.

9th. He was directed to continue the ſame diet, and to avoid much exerciſe; and the turgid ſtate of the veins of his hands admoniſhing me that his veſſels were ſtill too full, it occurred to me to adviſe, inſtead of a ſecond bleeding, that he ſhould drink as ſparingly as poſſible; from which I thought the veſſels would become leſs full, and the ruptured veſſel have a greater chance of uniting than when conſtantly diſtended by drinking; and that, if I could avoid taking away more blood, my patient would recover from his indiſpoſition much ſooner than if I reſted the chief ſtreſs of the cure upon this operation. He was, therefore, allowed only a pint of liquid, including tea and every other kind of drink, (all of which were given cold) in the twenty-four hours. When thirſty, I recommended it to him to ſuck an orange or lemon, inſtead of drinking. On former occaſions of this kind, viz. in active hæmorrhages, I have preſcribed (as is the common practice) cooling emulſions, milk whey and other diluents, in conſiderable quantities, with a view of relaxing the vaſcular ſyſtem, and thereby leſſening its increaſed

creaſed action, not conſidering that the ſtimulus of diſtention kept up this action, and was, therefore, one of the chief things I had to guard againſt. But as there is now little to be dreaded from the Boerhaavian lentor, ſo there is no particular occaſion for the great dilution commonly practiſed, and which ſeems to have been founded upon this doctrine. The medicines preſcribed this day were ſimilar to the former.

10th. I found him very cool, and without cough or expectoration of any kind. The pulſe was ſofter, leſs frequent, and in every reſpect better. The appearance of the cutaneous veins alſo was ſo different, that I was convinced this great alteration for the better was chiefly to be attributed to his having avoided much drinking during the preceding day and night. The draughts kept the body regularly open once or twice a day, and induced a ſoft ſkin and comfortable ſleep. They were, therefore, continued for three days, four every day; and three days more, two every day, ſtill obſerving the ſame rule as to drinking. They always produced the ſame ſalutary effects. From this time the patient was perfectly well, and has remained ſo ever ſince. In this caſe it would appear probable that no particular pneumonic affection exiſt-

ed, excepting the bleeding, which was moſt probably occaſioned by a plethoric ſtate of the conſtitution and particular determination to the lungs by the cough.

CASE II.

Soon after my attendance on the above patient, another caſe of hæmoptyſis occurred, but which differed from the former in being attended with conſiderable pneumonic affection beſides the hæmorrhage. The patient was a tall, thin man, about thirty years of age, of a pale complexion, narrow cheſt, and high ſhoulders, and had been affected with a ſevere cough for nearly four months previouſly to his application to me, accompanied with much yellow expectoration, and was ſuppoſed by his friends to be in a deep decline. He had no night ſweats; but for the laſt three weeks had been affected with a continual pain of the right ſide; which, as far as I could diſcover, did not originate from any rheumatic affection of the external muſcles, but from ſome internal diſeaſe of the thorax, and which I conceived to be a ſlow inflammation of the lungs, from which, and the violence of the cough, the hæmorrhage proceeded. He applied

applied to me in the beginning of April, when he was coughing violently, and bringing up blood in mouthfuls. He had conſiderable fever, with a full, hard pulſe. I took from him ten ounces of blood, and preſcribed in every reſpect as in the foregoing caſe, enjoining to him great attention not to drink more than a pint of liquid in twenty-four hours; this, and every other rule directed, he regularly obſerved for about three weeks, when the bleeding had ceaſed for three or four days, and alſo the pain in the ſide. But returning imprudently to his former diet, and drinking the uſual quantity as when in health, previouſly, as I ſuppoſe, to the obſtruction or inflammation of the lungs being removed, his cough returned, with ſome little appearance of bloody expectoration, mixed with that kind of yellow mucus, which is commonly diſcharged by mucous ſecreting ſurfaces when inflamed. Theſe ſymptoms, however, were entirely removed in the courſe of ten days, by a ſteady attention to the ſpare diet, and abſtinence from liquids, formerly recommended, and the medicines before uſed.

Since then he has been, and now is, in the moſt perfect health, without cough, pain in the ſide, or any other thoracic or pneumonic affection.

tion. It occurs to me that this ſecond attack, and the ſucceſs of the ſubſequent treatment, point out the delicate ſituation of the lungs, and alſo the efficacy of this method of cure.

Having related the above two caſes with every neceſſary preciſion, I ſhall beg leave to offer ſome few obſervations on active hæmorrhagy in general, and on that of the lungs in particular. In all active hæmorrhages a plethoric ſtate of the ſyſtem generally exiſts: all the blood veſſels of the body are full, diſtended, and acting vigorouſly; and hence, very commonly, rupture and conſequent hæmorrhage.

Therefore the chief proximate cauſe ſeems to be diſtention and conſequent increaſed action of the veſſels: Dr. Cullen, indeed, adds congeſtion of blood, which certainly may happen either from accidental determination of blood to a part, or ſome particular fault in the original conformation, or acquired relaxation, of the coats of the veſſels of certain parts. But it is well known that hæmorrhages may ariſe from general diſtention, without any particular congeſtion; and, in this caſe, will happen wherever the vaſcular ſyſtem is weakeſt or leaſt ſupported. The proximate cauſe being clearly aſcertained, the

the method of cure will appear obvious. Remove the preternatural diſtention of the veſſels, and their action will ſoon diminiſh; then nature, with very little aſſiſtance, will do the reſt. Although this is evidently the caſe, it appears ſingular that, hitherto, almoſt all practitioners have neglected the moſt effectual method of accompliſhing this deſirable purpoſe, viz by abſtinence from liquids. In Dr. Moffat's tranſlation of Aretæus, page 347, are the following words: "The drink ought to be very ſparingly exhibited, for moiſture is diſadvantageous in a dry "diet*." But, although this was written when treating of hæmorrhage, the intentions of Aretæus were only that the aſtringency (upon which he ſeemed to place his chief hope) of his diet might not be weakened by drinking.

The idea of moderate drinking is adopted by Dr. Rowley, in his treatiſe on "Female nervous diſeaſes," publiſhed in 1788. When treating of the "Immoderate flow of the menſes," page 32, he obſerves, "as hæmorrhages ſel-"dom happen, unleſs there be a ſufficient quan-"tity of blood in the body to rupture the veſ-

* This is an accurate interpretation of the paſſage in queſtion. The words of Aretæus are, ποτὸν δὲ τὸ ξύμπαν ὀλίγον ἔστω. ξηρῇ γὰρ διαίτῃ ὑγρὸν ἀξύμφορον.

"ſels,

" ſels, one principal part of the cure conſiſts in " not only obtaining, but preſerving a diminiſhed " quantity of blood, by a great *abſtinence* from " liquids; for by this means, the very ſources " of ſupply are cut off. If little be drank, the " blood veſſels which are, or have been, diſten- " ded beyond their proper dimenſions, will gra- " dually contract themſelves to their original " ſize, acquire ſtrength daily, and not having " ſo large a column of blood to circulate, they " will reſiſt the morbid diſpoſition of nature to " evacuate ſo violently the catamenia." But, the late celebrated Dr. Cullen, when treating on hæmoptyſis, particularly recommends, that " every part of the antiphlogiſtic regimen be " ſtrictly enjoined *," which includes " taking " in large quantities of mild antiſeptic liquors†:" and ſays, that the phlogiſtic diatheſis is to be taken off by bleeding, more or leſs, according to circumſtances. If, however, the ſtimulus of diſtention is kept up by filling the veſſels with liquids, the good effects of the bleeding are counteracted, and a frequent repetition rendered neceſſary—Whereas, if abſtinence from liquids

* Firſt Lines of the Practice of Phyſic, Vol. II. p. 353. 4th Edition.

† Ibid. Vol. I. p. 132.

be particularly attended to, one bleeding will have more effect, than three or four, if accompanied with that part of the antiphlogiſtic regimen, and the loſs of blood be thereby prevented; which, conſidering its importance in the conſtitution, and the difficulty with which its loſs is made up, ſhould be at all times avoided when poſſible.

Of all caſes of hæmorrhagy, that from the lungs is the moſt dangerous in its nature, and moſt difficult of cure. This will appear evident if we recollect their particular ſtucture, their large and numerous veſſels, their conſtant motion, &c.

As to their ſtructure, anatomy demonſtrates that they are compoſed of a congeries of blood veſſels, abſorbents, and nerves, together with the air cells; and that all theſe are only connected by the cellular membrane, the common connecting medium of the body: for I do not mention their pleuritic covering, as I am only ſpeaking of their ſubſtance. The blood veſſels, with which alone our preſent ſubject is connected, are very large, and in greater number than in any other part of the body of the ſame ſize. This was abſolutely neceſſary to circulate the very large quantity of blood generally ſent to them. Haller obſerves *, that the quantity of

* Prim. Lin. Phyſiol. § 246.

blood

blood which enters into the lungs is equal to, or even perhaps greater than, that which is ſent in the ſame time throughout the reſt of the body. And, as the chief buſineſs of the lungs is for reſpiration, by which they are kept conſtantly in action, ſo it will appear evident why hæmorrhages here are more dangerous, as well as more obſtinate, than in any other part, as their conſtant motion counteracts and prevents the union of the ruptured veſſel.

Queen Anne Street, Eaſt,
Auguſt 7, 1792.

XI. *An Account of a Diſeaſe which, until lately, proved fatal to a great Number of Infants in the Lying-in Hoſpital of Dublin; with Obſervations on its Cauſes and Prevention.* By Joſeph Clarke, *M. D. Maſter of the Hoſpital above mentioned, and M. R. I. A. — From the Tranſactions of the Royal Iriſh Academy*, 1789. 4to. Dublin, 1789.

LYING-IN Hoſpitals are inſtitutions of ſuch recent date, and ſo few in number, that hitherto we may conſider them as in a ſtate of infancy. Excepting ſome portion of the Hotel Dieu of Paris, which has been long allotted

lotted to the relief of poor pregnant women, I know of none that have exiſted above forty years, and very few can lay claim even to this antiquity. It can hardly appear unreaſonable, therefore, to ſuppoſe that imperfections ſtill exiſt in their management, which time and accurate compariſon may ſerve to detect: and although ſuch eſtabliſhments be at preſent confined to a few of the capital cities in Europe, it is probable their number will increaſe as their good effects in ſociety are experienced. It is hoped, therefore, that a few facts and obſervations, tending to point out a conſiderable ſource of error in an extenſive lying-in hoſpital, may be deemed worthy of public notice; both preſent and future inſtitutions of this nature may, perhaps, derive ſome uſeful information from ſuch enquiry.

Several years ago, in attempting to aſcertain the nature of the diſeaſe which is the ſubject of the following remarks, I found the doctrines contained in moſt medical books of very little uſe: all the morbid cauſes, commonly ſuppoſed to produce diſeaſes in infancy, appeared to me inadequate to an explanation of its phænomena. Doubts of courſe aroſe in my mind, ſome of which have been already ſtated to the public.

public *. At length I was tempted to hazard a conjecture, which then appeared probable, and which ſucceeding events ſeem to have confirmed. A ſketch of the evidence is here, with deference, ſubmitted to the candid conſideration of phyſicians, and of this Academy.

At the concluſion of the year 1782, of ſeventeen thouſand ſix hundred and fifty infants born alive in the Lying-in Hoſpital of this city, two thouſand nine hundred and forty-four had died within the firſt fortnight †, that is nearly every ſixth child, or about ſeventeen in the hundred. This was obviouſly a large proportion of deaths, as we ſhall prove more particularly hereafter. The diſeaſe which carried off moſt of theſe children, perhaps nineteen of twenty, was general convulſions, or what our nurſe-tenders have been long in the habit of calling the nine-day fits, as conſtantly occurring within the firſt nine days after birth. As this diſeaſe has hitherto yielded to no remedy, I have been always more engaged in attending to its prevention than cure. I am chiefly indebted for its hiſtory, therefore, to the united

* See Obſervations on the Properties commonly attributed by medical Writers to human Milk, &c. Tranſactions of the Royal Iriſh Academy, Vol. II. and London Medical Journal, Vol. XI.

† See abſtract of regiſtry at the end of this eſſay.

reports

reports of ſeveral of our moſt experienced nurſe-tenders. I took down their remarks ſeparately, and from the whole collected what follows.

In general it has been obſerved that ſuch children as are diſpoſed to whine and cry much from their birth, and ſuch as are ſubject to heavy deep ſleeps, or ſtartings in their ſleep, are peculiarly apt to fall into convulſive affections. Twiſting of the upper extremities, while awake, without any evident cauſe; a livid circle about the lips, and ſudden changes of colour in the countenance, have now and then been thought to portend the nine-day fits Screwing and gathering of the mouth into a purſe, accompanied at intervals with a particular kind of ſhrieking, well known to the experienced nurſe-tenders, are reckoned ſure, and by no means diſtant, forerunners. Sometimes previous to theſe ſymptoms, and ſometimes along with them, the infants are obſerved to be unuſually greedy for ſucking at the breaſt, or feeding by the ſpoon; laxatives given, in ſuch ſituations, ſeldom fail to operate freely, ſometimes bringing away greeniſh, ſlimy, or knotty ſtools; though not unfrequently they are of a natural yellow colour, as I myſelf have more than once ſeen.

Generally with one or more of these symptoms preceding, but sometimes without any warning whatever, the infants are seized with violent irregular contractions and relaxations of the muscular frame, but particularly of those of the extremities and face. These convulsive motions recur at uncertain intervals, and produce various effects. In some the agitation is very great; the mouth foams; the thumbs are riveted into the palms of the hands; the jaws are locked from the commencement, so as to prevent the actions of sucking and swallowing; and any attempts to wet the mouth or fauces, or to administer medicines, seem to aggravate the spasms very much; the face becomes turgid, and of a livid hue, as do most other parts of the body. From this circumstance, and from the shorter duration of the disease, when it occurs in this form, the nurses reckon this a different species, and call it the black fits. The conflict in such cases lasts from about eight to thirty hours, and in some very rare cases to about forty hours, when the powers of nature sink exhausted and overpowered, as it were, with their own exertions.

It much more frequently happens, however, that the spasmodic contractions are not so strong as above described; that the extremities are rather

ther twiſted than convulſed; that the power of ſucking, but more certainly of deglutition, is not loſt till near death; that the mouth foams leſs; and that the paroxyſms recurring at more diſtant intervals, continue to haraſs the patient from three to five days, and in ſome rare inſtances to ſeven and even nine. During all this period the face remains pale; and the body, from being perhaps very plump, is reduced to a moſt miſerable ſpectre by emaciation and diſeaſe. This the nurſes conſider as a ſecond ſpecies, and call it the white fits.

Both theſe ſuppoſed ſpecies, which may perhaps be more juſtly conſidered as varieties of the ſame diſeaſe, agree in conſtantly attacking within nine days from birth, and moſt frequently about the falling off of the umbilical chord. This is an event which generally takes place from the fourth to the ſixth or ſeventh day. Diarrhœa is a conſtant concomitant of both ſpecies. Long and ſad experience have found them alſo to be both equally fatal, inſomuch, that the memory of the oldeſt perſon does not furniſh an inſtance of one being cured.

In order to place my ideas of the cauſe of this fatal diſeaſe in the cleareſt point of view, I find it neceſſary to have recourſe to extracts

from a letter written by me in the year 1783 to the late Doctor Hutcheſon, who was then conſulting phyſician to the hoſpital in queſtion.

In this letter, which was written after having ſeen ſome of the beſt regulated Lying-in Hoſpitals in London, I ſtated to Doctor Hutcheſon,

That in an old hoſpital, which preceded the preſent, but inſtituted by and under the care of the ſame gentleman, and in a leſs airy part of Dublin, of three thouſand ſeven hundred and forty-ſix children therein born, only two hundred and forty-one died within the firſt month*, which are in the proportion of one to fifteen and a half, or from ſix to ſeven in the hundred.

That during a period of five or ſix years, in the Britiſh Lying-in Hoſpital, London, of three thouſand ſix hundred and eleven therein born, only one hundred and forty-ſix died, within the firſt three weeks or month, which are as one to twenty-five, or four in the hundred.

That in the London Lying-in Hoſpital I was poſitively aſſured that the death of an infant was

* See the caſe of Mr. Moſſe, offered to the conſideration of the Iriſh Houſe of Commons in the year 1755.

a rare

a rare occurrence. It is there computed with ſome confidence (for I was told that no written account is kept) that the number of ſtill-born infants far exceeds the number of thoſe dying after birth. The proportion of ſtill-born we know to be about a twentieth part, or five in the hundred.

That near forty years ago, when the diſeaſes of children were leſs underſtood, and more eſpecially the ſalutary practice of inoculation, Doctor Short computed from ſome very extenſive regiſters, that London loſt thirty-nine per cent. under the age of two years—Edinburgh and Northampton thirty-four or thirty-five—Sheffield twenty-eight—country places from twenty to twenty-eight;—whereas in the Dublin hoſpital there was loſt a number equal to half of that loſt in many of theſe places, and nearly equal to the whole of that in ſome of them, in two weeks, or in about the fiftieth part of the ſame ſpace of time. From which, and ſome other conſiderations of leſs weight, I thought the uncommon mortality of children in the Dublin Lying-in Hoſpital ſatisfactorily proved.

I then ventured to hazard ſome conjectures concerning the cauſes of a mortality, by which ſo many uſeful lives were loſt to the ſtate.

1ſt, Foul air, or an impure atmoſphere;

2d, Neglect of keeping the children clean and dry;

3d, Irregularity in the manner of living of their mothers, more eſpecially in the abuſe of ſpirituous liquors,—were the cauſes which appeared to me the moſt probable, either ſeparately or perhaps combined; but I ſuſpected that the firſt, viz. an impure or phlogiſticated atmoſphere, contributed moſt powerfully to the general calamity. For,

Firſt. I remarked to him that public regiſters proved the mortality of children to increaſe proportionably with the ſize of towns; and that the larger towns are, the more numerous are the cauſes which have a tendency to taint their atmoſphere, and thereby render it leſs fit for the purpoſes of ſalutary reſpiration.

Secondly. That in private practice phyſicians in the city of Dublin did not find the mortality of infants in any degree ſo conſiderable as our regiſtry proved it to be in the Hoſpital, a proof that there was here ſome peculiar exciting cauſe of diſeaſe.

Thirdly. That the difference between the mortality of the children in the old hoſpital and in the preſent one, when under the management

of

of the ſame eminent character, Mr. Moſſe, afforded the ſtrongeſt evidence in favour of this conjecture. Such difference could not be ſuppoſed to ariſe from any different method of feeding or cloathing them, or in the exhibition of medicines; to me it ſeemed to originate from a difference in the apartments and accommodations of the women. In the former, which was an old houſe, and never deſigned for an hoſpital, were one or two, or at moſt three beds in a room, to each of which there muſt have been a door, and one or two, perhaps three windows; whereas in the latter were eight beds in the ſame room, and only one door properly ſpeaking *, with three windows in ſome, and two in others; whence it is evident that the ſupply of freſh air in each being nearly on an equality, it muſt be much ſooner corrupted by the reſpiration, lochial diſcharges, and other effluvia of eight

* There is indeed a ſecond door to each of our large wards; but as it opens into a ſmall ward, containing two beds, it is probable the air derived from ſuch communication is not very ſalubrious. The dimenſions of our large wards, in the front of the hoſpital, are 36 feet by 23, and 13 in height: in the rear $33\frac{1}{2}$ by 23, and of equal height. The ſmall wards in front are 19 by $12\frac{1}{2}$; and in rear, 18 by $13\frac{1}{2}$.

women and as many children, than by thoſe of two or three.

Fourthly. I obſerved, in farther confirmation of this doctrine, that the Britiſh Lying-in Hoſpital in London, which is very favourable to the lives of infants, was an old building, which ſeemed not to have been originally deſigned for an hoſpital; in it there were but ſix beds in a room with one door, one ſmall and three large windows, with a ventilator to each of the latter; that their beds had curtains, but no canopies as in Dublin, and that the utmoſt cleanlineſs was in every reſpect obſerved. That in the London Lying-in Hoſpital, which is an elegant modern building, there are but ſeven beds to a ward, with two large and four ſmall windows to each, one door with a large ventilator over it, the ceilings lofty and perforated by an air-pipe of ſeveral inches diameter, which paſſes out at ſome part of the roof. Here alſo the moſt ſcrupulous cleanlineſs is obſerved, and large ſupplies of clean linen given both for beds, women and infants; and here the death of an infant is a rare occurrence.

Laſtly. I alledged it was by no means inconſiſtent with analogy or reaſon to ſuppoſe that the accumulated effluvia ariſing from the bodies of puerperal

puerperal women and children in lying-in hoſpitals might acquire qualities peculiarly noxious to the delicate frame of infants. That in other hoſpitals and gaols, as the pernicious effects of accumulated human effluvia have been often experienced by robuſt adults, it is poſſible that degrees of contagion inferior to theſe may prove fatal to infants. I concluded with quoting the authority of Arbuthnot, who has obſerved "that "the air of cities is very unfriendly to infants "and children; for that as every animal is adap-"ted by nature to the uſe of freſh and free air, "the tolerance of air replete with ſulphureous "ſteams of fuel and the perſpirable matter of "animals (as that of cities) is the effect of "habit which young creatures have not yet ex-"perienced*;" and that if the air of cities be unfriendly, *a fortiori*, ſo muſt the air of hoſpitals in cities, and that in proportion to their want of ventilation.

To theſe reaſons I might have added, on the authority of Doctor Prieſtley, that healthy animals almoſt always die of convulſions on being put into air in which other animals have died, after breathing it as long as they could; and that moſt other kinds of air, noxious to animal

* Eſſay concerning the Effects of Air on human Bodies.

life,

life, produce ſimilar effects. See Experiments and Obſervations on different Kinds of Air, Vol. I. page 71.

Viewing the ſubject in this light, I propoſed a number of alterations intended for the more complete ventilation of the hoſpital, and for which I was principally indebted to Mr. White's excellent work on the management of lying-in women. My obſervations had the effect I wiſhed with Doctor Hutcheſon and the medical governors. Apertures of a conſiderable ſize were made in the ceilings of each ward, which have been ſince changed for air pipes of ſix inches diameter. Three holes, of an inch diameter, were bored, in an oblique direction, through each window frame at top. The upper parts of the doors, opening into the gallery, were alſo perforated with a great number of holes. By theſe means a free and eaſy paſſage was given to the air through the wards at all times, and executed in ſuch a manner as to put it out of the power of nurſe-tenders or patients to control it. Since the above period alſo the number of beds in the large wards have been reduced to ſeven, and ſeveral changes made in their conſtruction, which render them more airy, and more eaſily kept clean. The conſequences have been fa-

vourable far beyond the expectation of every perſon concerned. The nine-day fits are become viſibly leſs frequent; and the abſtract of our regiſtry ſhews the fact at firſt view to the moſt inattentive obſerver. Of eight thouſand and thirty-three children born ſince the above period, only four hundred and nineteen have died in the hoſpital; that is nearly one in nineteen and a third, or from five to ſix in the hundred. Had the mortality of infants been in this proportion ſince the commencement of the Dublin hoſpital, the number of children dead would have been ſomewhat about thirteen hundred, inſtead of the preſent number, three thouſand three hundred and ſixty-three; or in other words, above two thouſand lives would have been ſaved to the community.

That this diminution of mortality is to be attributed to improvements in ventilation can admit, I think, of little doubt. No other new mode of management has been of late practiſed to account for it. No other remedies uſed than ſuch as have been tried a thouſand times unſucceſsfully. I know it has been objected, that it may be owing to their mothers now remaining a ſhorter ſpace of time in the hoſpital than formerly. In order to aſcertain whether this

this be a matter of fact, I have, for the laſt two years, had an entry made of the day on which each infant died; the number dead has been one hundred and fourteen, and they have died on the following days after their birth:

12th day,	11th,	10th,	9th,	8th,	7th,	6th,	5th,	4th,	3d,	2d,	1ſt.	Total.
1 died.	0.	3.	3.	5.	24.	37.	18.	6.	5.	10.	2.	114 died.

Hence it is obvious that the fatal days are the fifth, the ſeventh, but eſpecially the ſixth; and either of theſe are undoubtedly much within the average day of the diſcharge of our patients. Beſides, the early diſcharge of patients did not commence in any one year, as the leſſened mortality of infants did; it aroſe from gradual increaſe in the number of poor demanding admiſſion; and I am happy to add, that ſome late very liberal donations, and a conſequent increaſe in the number of our beds, have put an end to the neceſſity of this diſagreeable expedient, adopted ſolely with a view of affording indiſcriminate relief.

It might naturally be ſuppoſed that an atmoſphere, which we have endeavoured to prove injurious to the health of infants, would alſo ſomewhat affect the chances of life in their mothers. The fact, however, certainly is, that on an average fewer women have died in child-

bed

bed in the Dublin hoſpital than in moſt other lying-in hoſpitals, (Compare the abſtract at the end of this eſſay with facts contained in the poſtſcript to Mr. White's treatiſe on the management of pregnant and puerperal women.) Here then a queſtion ariſes, why ſhould infants be ſo much more liable to injury from an impure atmoſphere than adults? Is it poſſible that mothers ſhall eſcape with impunity and their children periſh? This, I own, puzzled me extremely, and had almoſt made me doubt of what I conſidered a fact, ſupported by the ſtrongeſt probable evidence. By accident, however, in looking over a diſſertation on the food and diſcharges of the human body, by our celebrated countryman, the late Doctor Bryan Robinſon, I found ſome facts and obſervations which appear to me to go a great way towards an explanation.

In order to make theſe facts intelligible to perſons not very converſant in ſuch ſpeculations, I muſt premiſe, that Doctor Prieſtley has fully proved one great and indiſpenſable uſe of reſpiration to be to carry off or leſſen a certain quality in the blood, which is known by the name of phlogiſton. That this can only be done by pure air. That by the addition of phlogiſton

to

to blood it acquires a deep black colour; and by its avolation, that blood returns to its natural florid hue.

Now Doctor Robinson found by experiment*, that the weight of the heart, in respect to the weight of the body, is greater in children than in grown bodies, and that their quantity of blood is proportional to the weight of the heart. He found also, that the quantity of blood, which flows through the lungs in a given time, in proportion to the mass of circulating fluids, is greater in children than in grown bodies; and that this proportion lessens continually from the birth till bodies arrive at their growth. Hence he remarks, that as the blood of children passes oftener through the lungs, it is more fluid and of a brighter colour than the blood of grown persons.

If this be a true picture of the constitution of infants, we must presume that such peculiarities are intended to answer some very important purposes in the œconomy of young animals; and that in proportion as the intention of Nature is in these respects frustrated, the effects will be more or less severely felt. Would it be deem-

* See page 13, et seq. of his work.

ed

ed a conjecture, exceeding the bounds of probability, to ſuſpect that the avolation of a very large quantity of phlogiſton, and its due ſeparation from the maſs of blood by pure air, may be eſſentially neceſſary to the growth of young animals; and that this may be one reaſon why the impure air of cities has, in all ages, been particularly deſtructive to their health?

With a view of reducing the nine day fits to its proper genus and ſpecies in noſology, I have turned over the works of ſome of our beſt writers on this ſubject. The only genus to which I think it can with any propriety be reduced, is that of *eclampſia* or *convulſion des enfans* of Sauvages. But although under this generic title he deſcribes ſeventeen ſpecies, there is not one of them to which it bears an exact reſemblance. The *eclampſia neophytorum* of Vander Monde is widely different, as any one may eaſily ſee by caſting an eye over the hiſtory of both. As moſt of the ſpecies enumerated by Sauvages are ſymptomatic, and as he diſtinguiſhes ſeveral of them from various kinds of deleterious ſubſtances taken into the ſyſtem; as *eclampſia ab atropa, cicuta*, &c. perhaps we might with equal propriety add *eclampſia ab atmoſphæra phlogiſticata*.

There

There is a ſporadic diſeaſe in Minorca and ſome other countries ſo very like the nine day fits, in ſome particulars, that it may be worth while here to collect, under one point of view, a few extracts concerning it. Noſologiſts have given it the title of triſmus naſcentium. " In hac urbe " afflictantur plurimi infantes, adeo feroci con- " vulſione mandibulæ inferioris, ut ea appre- " henſi, nullo poſſint motu illam movere, et " abhinc ſuctus lactis impeditur omnino.....Tot " interficit mala iſta convulſio, ac variolæ aut " morbilli.....In hoc periculum incurrunt recen- " ter nati uſque ad *nonum* ſuæ nativitatis diem, " eoque tranſacto, omne diſcrimen ceſſare do- " cuit ſemper experientia." For theſe and ſome other obſervations, from the writings of a Spaniſh phyſician, we are indebted to my friend Doctor Cleghorn's valuable treatiſe on the diſeaſes of Minorca. After the hiſtory of the diſeaſe, the doctor obſerves that is is needleſs to add the remedies preſcribed by the Spaniſh author, as he ingenuouſly confeſſes the diſeaſe to be ſo ſeldom curable, that in twenty years practiſe he had ſcarce known ſix to recover.

In Germany, Heiſter, de maxillæ ſpaſmo, obſerves, " Quod ſi ſponte, ſive e cauſa interna, " hic maxillæ ſpaſmus in infantibus, ut ſæpe " vidi,

" vidi, contingit ut plurimum moriuntur et " vix ullum ſervatum vidi ; licet laudatiſſima " remedia nervina et antiſpaſmodica internè " atque externè quam ſolertiſſimè adhibita fue- " rint."

Hofer, in the firſt volume of the Acta Helvetica, has given a long account of a diſeaſe not unfrequent in ſome parts of Switzerland, which Sauvages and Cullen ſeem to think of the ſame ſpecies with the preceding, but which differs from them very materially in ſome reſpects.— The title of his paper is, De tetano maxillæ inferioris in Infantibus. " Subjectum iſti ob- " noxium," ſays he, " eſt infans, qui inter " tertiam et *duodecimam* ætatis diem verſa- " tur. Cura hujus morbi, quamvis valde " lenta ſit, attamen ſi infans quintam a mor- " bi invaſione diem tranſegerit certiſſime fe- " lix eſt, ideoque dummodo tempus terere " poſſumus, res in ſalvo poſita eſt." After giving an account of his method of cure, which conſiſts of a farrago of diſtilled waters, ſyrups and inert powders, as may be ſeen in Sauvages, he concludes, " hæc eſt methodus applicando- " rum medicamentorum, quâ ex tribus ægro-

" tulis curæ meæ commiſſis plerumque unus " gratiâ divinâ evaſit."

A late French author, Monſ. Fourcroy, in a treatiſe entitled Les Enfans elevés dans l'ordre de la Nature, remarks " Quand je ſuis arrivé " en 1744 a St. Domingue, on ne pouvoit ele- " ver des negrillons dans la plaine du Cap Fran- " cois. Ils mouroient preſque tous, c'eſt a " dire environ quatre vingt ſur cent, d'une ma- " ladie appellée dans le pays *mal de machoire* " ou *tetanos*, qui les emportoit dans les *neuf* " premiers jours de leur naiſſance." This diſ- order he informs us, when come on, is beyond the power of medicine, but that much may be done in the way of prevention.

From theſe obſervations it is evident,

That in certain parts of the world children are more ſubject to ſpaſmodic diſeaſes than others.

That theſe are more apt to come on within nine days after birth.

That coming on within this period they are generally productive of the moſt fatal effects.

And

And laſtly,

That their cauſes and cure are ever involved in obſcurity.

In each of theſe particulars, there is a ſtriking analogy between the triſmus naſcentium or tetanus maxillæ inferioris and the nine-day fits.

It is farther worthy of obſervation, that the diſorders of adults, which are confined to particular diſtricts or tracts of country, more frequently ariſe from ſomething noxious infecting the atmoſphere of ſuch places than from any other cauſe; and however difficult it may be to apply this doctrine to the caſes in queſtion, it at leaſt affords ſome probable evidence towards the ſuppoſition, that they originate from ſomewhat ſimilar cauſes.

Such are the obſervations which reflection and ſome reading ſuggeſted to me on this ſubject, previous to the publication of the London Medical Tranſactions in the year 1785. In this very excellent work, however, I met with "An account of a ſingular diſeaſe which pre-

" vailed among ſome poor children maintained " by the pariſh of St. James in Weſtminſter;" which appears to me to throw much light on this obſcure ſubject: I hope to be excuſed, therefore, for making ſome extracts from this valuable eſſay, for which the world is indebted to the accurate and learned Sir George Baker.

Sir George informs us, that on the 24th day of September, 1782, ſeventy-three children, viz. forty-ſix girls and twenty-ſeven boys, of different ages, from that of ſeven to fourteen years, were removed from Wimbleton to a large houſe near Golden-Square. To this houſe theſe children came in good health, and continued ſo for a fortnight; when on the 8th of October, a girl aged thirteen years was ſuddenly ſeized with an excruciating pain in the region of the ſtomach and in the back, which was ſoon followed by violent head-ach, delirium and convulſions. After a few days, another and another girl were attacked exactly in the ſame manner; and towards the end of the month this diſeaſe had ſo prevailed as very much to alarm all thoſe to whom the care of theſe children had been committed. On the 29th day of October Sir George's

George's advice was desired. He found nine of these poor girls and a female servant in the same room suffering the various effects of a most dreadful malady. Five were in the agonies of extreme pain, three were most cruelly convulsed, and the other two were raving in a fit of delirium. The other inhabitants of this house had in general been healthy during the month of October, and it is remarkable that the disease above described affected females only, and was confined to those who had slept together in a certain room on the second floor. The height of this room was a little more than eight feet, the length twenty, and the breadth sixteen: it contained ten beds, in which it was intended that eighteen girls, two in each bed, and a female servant singly should sleep; but Sir George discovered that this being a favourite room on account of its warmth, was generally crouded at night by a much greater number than its complement: that as much space as possible might be made for beds, the chimney had been stopped up with bricks, and it had been the constant custom of the servant at night to keep the door shut and to close the window shutters, that as little fresh air as possible might be admitted

On enquiry it appeared that three candles and a lamp of oil had been generally uſed during the night in this chamber, but they were hardly of any ſervice, giving a glimmering light and frequently almoſt extinguiſhed.

Sir George adviſed the chamber of the ſick to be evacuated without delay, the healthy to be ſeparated from the diſeaſed, the chimney to be opened, and whatever tended to exclude freſh air to be removed. Theſe directions were complied with, and the patients having been removed to a large apartment (where proper care was taken that freſh air might be admitted) paſſed a quiet night free from every ſymptom of the diſeaſe. However, the next morning, immediately on their awaking, they were all ſeized in the uſual manner, but it was very ſoon obſervable, that the paroxyſms returned leſs often and with leſs violence, and ſometimes without convulſions, and that during the intervals the delirium appeared gradually to abate.

From theſe and various other important facts which we cannot here recite, Sir George conjectures that the ſource of this extraordinary diſeaſe was vitiated air. To me his evidence appears ſufficient to afford conviction to every reaſonable

reaſonable mind, and if I am not miſtaken, it adds greatly to the probability of the opinion, which ſuppoſes that the nine-day fits originated from a ſimilar ſource.

Upon the whole, from the evidence adduced, I hope the following inferences may not appear improbable.

1. That one effect of an impure atmoſphere, on the human body, is to produce ſpaſms and convulſions.

2. That all young creatures, and eſpecially infants within nine days after birth, ſuffer moſt ſeverely by ſuch a noxious cauſe; and therefore

3. That in the conſtruction of lying-in hoſpitals, and perhaps of all public buildings intended for the reception of children, lofty ceilings, large windows and moderate ſized rooms ſhould be eſpecially attended to.

4. That in the arrangement of ſuch edifices, no apartment ſhould be completely filled with beds, if it can be conveniently avoided; and

5. That in their management attention is eſpecially neceſſary to cleanlineſs, as well as to

the conſtant and uniform admiſſion of atmoſpheric air by night as well as by day; and

Laſtly. That by purſuing ſuch meaſures with care, diſeaſes may be prevented which it has hitherto been found difficult, and ſometimes impoſſible, to cure.

XII. *Ob-*

[To face page 104.

An Abstract of the Registry * kept at the Lying-in Hospital, in Dublin,

From the 8th of December, 1757, (the Day it was first opened) to the 31st of December, 1788, each Year distinguished.

By B. H. Register.

		Number of Patients admitted.	Went out not delivered.	Delivered in the Hospital.	Boys born.	Girls born.	Total Number of Children.	Women having twins.	Children dead.	Children still-born.	Women dead.
From 8th to 31st December	1757	55	—	55	30	25	55	—	6	3	1
Year ending 31st of December	1758	455	1	454	255	207	462	8	54	21	8
	1759	413	7	406	228	192	420	13 1 had 3	95	22	5
	1760	571	15	556	300	260	560	4	116	36	4
	1761	537	16	521	283	249	532	11	104	29	9
	1762	550	17	533	279	266	545	12	106	33	6
	1763	519	31	488	274	224	498	12	94	29	9
	1764	610	22	588	287	308	595	7	83	28	12
	1765	559	26	533	288	251	539	6	94	25	6
	1766	611	30	581	324	261	585	4	111	18	3
	1767	695	31	664	373	301	674	10	125	29	11
	1768	689	34	655	362	302	664	9	154	47	16
	1769	675	33	642	350	301	651	9	152	38	8
	1770	705	35	670	372	305	677	7	107	37	8
	1771	724	29	695	370	341	711	16	102	44	5
	1772	725	21	704	368	344	712	8	116	32	4
	1773	727	33	694	367	344	711	17	136	31	13
	1774	709	28	681	357	334	691	10	154	29	21
	1775	752	24	728	364	378	742	14	122	27	5
	1776	883	31	802	418	407	825	22 1 had 3	132	39	7
	1777	872	37	835	452	395	847	12	145	35	7
	1778	961	34	927	476	460	936	9	127	39	10
	1779	1064	53	1011	550	476	1026	15	146	59	8
	1780	967	48	919	499	441	940	21	115	41	5
	1781	1079	52	1027	598	447	1045	18	121	38	6
	1782	1021	31	990	549	458	1007	17	127	57	6
	1783	1230	63	1167	632	553	1185	17 1 had 3	91	72	15
	1784	1317	56	1261	643	641	1284	24	76	68	11
	1785	1349	57	1292	711	609	1320	28 1 had 3	87	75	8
	1786	1396	45	1351	716	656	1372	21	51	101	8
	1787	1418	71	1347	705	670	1375	28	59	95	14
	1788	1533	64	1469	725	771	1496	25 1 had 4	55	72	23
Totals		26321	1075	25246	13505	12177	25682	432	3363	1349	282

Proportion of males and females born, about *nine* males to *eight* females.
——————— children dying in the hospital, as *one* to about *seven*.
——————— children still born, as *one* to about *nineteen*.
——————— women having twins (and more), as *one* to about *fifty eight*.
——————— women dying in childbed, as *one* to about *ninety*.
——————— women having *three* (and *four*) children, as *one* to about *five thousand and fifty*.

* An abstract of this Registry, from 1757 to 1784, was annexed by Dr. Clarke to his letters to the late Dr. Price, (see Philosophical Transactions, Vol. LXXVI. and London Medical Journal, Vol. IX.); but the present abstract, being brought down to a later period, includes a much greater number of facts, and, in particular, shows the decreased mortality of children in the Hospital since the year 1783.—Editor.

XII. *Observations on certain horny Excrescences of the human Body. By* Everard Home, *Esq. F. R. S.*—Vide *Philosophical Transactions of the Royal Society of London, Vol.* LXXXI. *for the Year* 1791. *Part* I. 4to. London, 1791.

WE have here an account of a disease, very remarkable in its effects, but very little understood as to its cause, namely, the production of an excrescence similar to horn. Excrescences of this sort, arising from the human body, have sometimes been met with in this and other countries; and the horns themselves have been deposited as valuable curiosities in different collections in Europe.

Mr. Home observes, that, in giving the history of a disease so rare in its occurrence, and in its effects so remarkable as almost to exceed belief, it might be thought right to take some pains in bringing proofs to ascertain that such a disease does really exist; but he considers the doing so as less necessary at present, there being two women now alive, and residing in England, who are affected by this complaint. Of these two cases we have here a very full and distinct account,

account, which ſhows the progreſs of the diſeaſe through its different ſtages.

The ſubject of the firſt of theſe caſes is Mrs. Lonſdale, fifty-ſix years old, and a native of Horncaſtle in Lincolnſhire, who, fourteen years ago, obſerved a moveable tumor on the left ſide of her head, about two inches above the upper arch of the left ear, which gradually increaſed in the courſe of four or five years to the ſize of a pullet's egg, when it burſt, and for a week continued to diſcharge a thick, gritty fluid. In the center of the tumor, after the fluid was diſcharged, ſhe perceived a ſmall ſoft ſubſtance, of the ſize of a pea, and of a reddiſh colour on the top, which at that time ſhe took for proud fleſh. It gradually increaſed in length and thickneſs, and continued pliable for about three months, when it firſt began to put on an horny appearance. In two years and three months from its firſt formation, made deſperate by the increaſed violence of the pain, ſhe attempted to tear it from her head; and with much difficulty, and many efforts, at length broke it in the middle, and afterwards tore the root from her head, leaving a conſiderable depreſſion which ſtill remains in the part where it grew. Its length altogether, it ſeems, is about five inches,

and its circumference at the two ends about one inch; but in the middle rather leſs. It is curled like a ram's horn contorted, and in colour much reſembles iſinglaſs.

From the lower edge of the depreſſion another horn, we are told, is now growing, of the ſame colour with the former, in length about three inches, and nearly the thickneſs of a ſmall gooſe quill; this is leſs contorted than the other, and lies cloſe upon the head.

A third horn, ſituated about the upper part of the lambdoidal ſuture, is much curved, above an inch in length, and more in circumference at its root: its direction is backwards, with ſome elevation from the head. At this place two or three ſucceſſive horns have been produced, which ſhe has conſtantly torn away; but, as freſh ones have ſpeedily followed, ſhe leaves the preſent one unmoleſted in hopes of its dropping off.

Beſides theſe horny excreſcences, there are two tumors, each of the ſize of a large cockle; one upon the upper part, the other about the middle of the left ſide of the head; both of them admit of conſiderable motion, and ſeem to contain fluids of unequal conſiſtence; the upper

upper one affording an obſcure fluctuation, the other a very evident one.

The four horns were all preceded by the ſame kind of encyſted tumors, and the fluid in all of them was gritty; the openings from which the matter iſſued were very ſmall, the cyſts collapſed and dried up, leaving the ſubſtance from which the horn proceeded diſtinguiſhable at the bottom. Theſe cyſts gave little pain till the horns began to ſhoot, and then became very diſtreſſing, and continued with ſhort intervals till they were removed.

This caſe, we are told, was drawn up by the ſurgeon who attended the patient for many years, and who of courſe had frequent opportunities of ſeeing the diſeaſe in its different ſtages, and acquiring an accurate hiſtory of its ſymptoms.

The ſubject of the ſecond caſe is a middle-aged woman, of the name of Allen, reſident in Leiceſterſhire, and who had an encyſted tumor upon her head, immediately under the ſcalp, very moveable, and evidently containing a fluid. It gave no pain unleſs preſſed upon, and grew to the ſize of a ſmall hen's egg. A few years ago it burſt, and diſcharged a fluid; this diminiſhed in quantity, and in a ſhort time

a horny

a horny excreſcence, ſimilar to thoſe above mentioned, grew out from the orifice, which has continued to increaſe in ſize; and in the month of November 1790, when Mr. Home ſaw it, was about five inches long, and a little more than an inch in circumference at its baſe. It was a good deal contorted, and its ſurface was very irregular, having a laminated appearance. It moved readily with the ſcalp, and ſeemed to give no pain upon motion; but, when much handled, the ſurrounding ſkin became inflamed. This woman came to London, and exhibited herſelf as a ſhow for money.

That theſe two caſes may not be conſidered as peculiar inſtances from which no concluſions can be drawn, Mr. Home has thought it right to take notice of ſome of the moſt remarkable hiſtories of this kind that are to be met with in books, and to conſider how far they agree with thoſe he has ſtated, in the general characters that are ſufficiently obvious to ſtrike a common obſerver; for the vague and indefinite terms in which authors expreſs themſelves on this ſubject ſhow plainly, he thinks, that they did not underſtand the nature of the diſeaſe, and their accounts of it, he obſerves, are not very ſatisfactory to their readers.

In

In the *Ephemerides Academiæ Naturæ Curiosorum* he has found two cases of horns growing from the human body. One of these instances was a German woman *, who had several swellings, or ganglions, upon different parts of her head, from one of which a horn grew. The other was a nobleman †, who had a small tumor, about the size of a nut, growing upon the parts covering the two last or lowermost vertebræ of the back. It continued for ten years, without undergoing any apparent change; but afterwards enlarged in size, and a horny excrescence grew out from it.

In the History of the Royal Medical Society at Paris ‡, he has met with an account of a woman, 97 years old, who had several tumors on her head, which had been 14 years in growing to the state they were in at that time: she had also a horn which had originated from a similar tumor. The horn was very moveable, being attached to the scalp, without any adhe-

* Ephem. Acad. Nat. Cur. Dec. iii. An. V. Append. p. 148.

† Ibid. Dec. i. An. I. Observat. 30.

‡ Histoire de la Société Royale de Médecine, 1776, p. 316.

sion

ſion to the ſcull. It was ſawn off, but grew again, and although the operation was repeated ſeveral times, the horn always returned.

Bartholine *, he obſerves, takes notice of a woman who had a tumour under the ſcalp, covering the temporal muſcle. This gradually enlarged, and a horn grew from it, which had become twelve inches long in the year 1646, when it was ſeen by Bartholine, who has given a repreſentation of it, which, Mr. Home tells us, bears a very accurate reſemblance to that which he has mentioned to have ſeen in November, 1790. No tumour or ſwelling is expreſſed in the figure; but the horn is coming directly out from the ſurface of the ſkin.

Mr. Home's next reference is to Leigh's Natural Hiſtory of Cheſhire, where a woman is mentioned to have lived in the year 1668, who had a tumor or wen upon her head for thirty-two years, which afterwards enlarged, and two horns grew out of it; ſhe was then ſeventy-two years old.

There is a horny excreſcence in the Britiſh Muſeum, which is eleven inches long, and two

* *Vide* Thomæ Bartholini Hiſtor. anat. rar. cent. 1. hiſt. 78.

inches

inches and a half in circumference at the baſe, or thickeſt part. The following account of this horn, taken from the records of the Muſeum, was given to our author by Dr. Gray. "A woman, "named French, who lived near Tenterden, "had a tumour or wen upon her head, which "increaſed to the ſize of a walnut; and in the "forty-eighth year of her age this horn began "to grow, and in four years arrived at its pre-"ſent ſize *."

There are many ſimilar hiſtories of theſe horny excreſcences, our author obſerves, in the books he has quoted, and in ſeveral others; but thoſe mentioned above are the moſt accurate and particular with reſpect to their growth, and in all of them we find the origin was from a tumour,

* In a note to this part of his paper Mr. Home gives the following extract from the Minutes of the Royal Society, Feb. 14, 1704-5.

"A Letter was read from Dr. Chariere, at Barnſtaple, "concerning a horn, ſeven inches long, cut off the ſecond "vertebra of the neck of a woman in that neighbour-"hood.

"Dr. Gregory ſaid, that one of ſeven inches long, and "of a dark brown colour, was cut off from a woman's "temple at Edinburgh.

"Dr. Norris ſaid, that two horns had been cut off from a woman's head in Cheſhire."

as

as in the two cases he has related; and although the nature of the tumour is not particularly mentioned, there can be no doubt, he thinks, of its having been of the encysted kind, since in its progress it exactly resembled them, remaining stationary for a long time, and then coming forwards to the skin; and the horn being much smaller than the tumour previously to the formation of the horn, is a proof, he observes, that the tumour must have burst, and discharged its contents.

From the foregoing account it will, he thinks, appear evident, that these horny excrescences are not to be ranked among the appearances called *lusus naturæ*: nor are they, in his opinion, altogether the product of disease, although undoubtedly the consequence of a local disease which has previously existed; they are, he contends, more properly speaking, the result of certain operations in the part for its own restoration; but the actions of the animal œconomy being unable to bring them back to their original state, this species of excrescence is formed as a substitute for the natural cuticular covering.

To explain the manner in which these horns are formed, he has thought it necessary to consider the nature of encysted tumours a little more

fully; and in doing ſo he makes it appear, that this particular ſpecies does not differ in its principle, nor materially in its effects, from many others which are not uncommonly met with in the human body, as well as in thoſe of many other animals, which, as they are more frequent in their occurrence, are alſo much better underſtood.

Encyſted tumours, he obſerves, differ exceedingly among themſelves, both in the nature of their contents, and in their progreſs towards the external ſurface of the body. Many of them have no reference to our preſent purpoſe: it is only the more indolent kind to which he means now to advert; ſome of theſe, he remarks, when examined, are not found to contain a fluid, but a ſmall quantity of thick, curd-like matter, mixed with cuticle broken down into ſmall parts, and, upon expoſing the internal ſurface of the cyſt, it is found to have an uniform cuticular covering adhering to it, ſimilar to that of the cutis on the ſurface of the body, from which it only differs in being thinner, and more delicate, bearing a greater reſemblance to that which covers the lips. Others of this kind, inſtead of having cuticle for their contents, are filled with hair mixed with a curdled ſubſtance, or hair without

without any admixture whatever, and have a similar kind of hair growing upon their internal surface, which is likewise covered with a cuticle. These cuticular encysted tumours were, he believes, first accurately examined by Mr. Hunter, to whom we are likewise indebted for an explanation of the mode in which the parts acquire this particular structure.

Mr. Hunter, it seems, considers the internal surface of the cyst to be so circumstanced respecting the body as to lose the stimulus of being an internal part, and receive the same impression from its contents, either from their nature, or the length of application, as the surface of the skin does from its external situation. It, therefore, takes on actions suited to such stimuli, undergoes a change in its structure, and acquires a disposition similar to the cutis, and is consequently possessed of the power of producing cuticle and hair. What the mode of action is, by which this change is brought about, is not easily determined; but from the indolence of these complaints, it most probably requires a considerable length of time to produce it. That the lining of the cyst really does possess powers similar to cutis, is proved, our author thinks, by the following circumstances: — that it has a

power of forming a ſucceſſion of cuticles like the common ſkin; and what is thrown off in this way is found in the cavity of the cyſt. It has a ſimilar power, he obſerves, reſpecting hair, and ſometimes the cavity is filled with it, ſo great a quantity has been ſhed by the internal ſurface. Beſides theſe circumſtances, the hair found in the cyſt correſponds in appearance with that which grows upon the body of the animal; and when encyſted tumours of this kind form in ſheep, they contain wool. What is ſtill more curious, when ſuch cyſts are laid open, the internal ſurface undergoes no change from expoſure, the cut edges cicatriſe, and the bottom of the bag remains ever after an external ſurface. Different ſpecimens, we are told, illuſtrative of the above-mentioned circumſtances, are preſerved in Mr. Hunter's collection of diſeaſes.

The cyſts that produce horny excreſcences (which are only another modification of cuticle) are, in our author's opinion, very improperly conſidered as giving riſe to horns; for if we examine the mode in which this ſubſtance grows, we ſhall find it, he obſerves, the ſame with the human nails, coming directly out from the ſurface of the cutis. It differs from the

nails

nails in not being ſet upon the ſkin by a thin edge, but by a ſurface of ſome breadth, with a hollow in the middle, exactly in the ſame manner as the horn of the rhinoceros*; at leaſt this, he aſſures us, is evidently the caſe in the ſpecimen preſerved in the Britiſh Muſeum, and in one which grew out from the tip of a ſheep's ear; they are alſo, he adds, ſolid, or nearly ſo, in their ſubſtance.

This mode of growth, our author obſerves, is very different from that of horns, which are all formed upon a core, either of bone or ſoft parts, by which means they have a cavity in them, a ſtructure peculiar to this kind of cuticular ſubſtance.

Encyſted tumours in different animals would appear, he thinks, from theſe obſervations, to be confined in their production to the cuticular ſubſtance proper to the animal in which they take place; for, although cuticle, hair, nail, hoof, and horn, are equally productions of ani-

* "The horn of the rhinoceros is a cuticular appendage to the ſkin, ſimilar to nails and other cuticular excreſcences, being in no reſpect allied to horns but in the external appearance."—*Note by Mr. Howe.*

mal ſubſtance, only differing in trivial circumſtances from each other, we do not, he remarks, find in the human ſubject any inſtance of an encyſted tumour containing a ſubſtance different from the cuticle, hair, and nails of the human body, to which laſt the horny excreſcences, the ſubject of the preſent paper, are, according to our author's obſervations, very cloſely allied, both in growth, ſtructure, and external appearance; and when of ſome length, they are found, we are told, to be ſo brittle as to break in two, upon being roughly handled, which could not happen either to hoof or horn. In the ſheep, he obſerves, they produce wool inſtead of hair; and in one inſtance in that animal, where they gave riſe to an horny excreſcence, he found it leſs compact in its texture, and leſs brittle than ſimilar appearances in the human ſubject; upon being divided longitudinally, the cut ſurface had more the appearance of hoof, and was more varied in colour, than nail.

Encyſted tumours being capable of producing horns, upon the principle here laid down, is contrary, Mr. Home obſerves, to the uſual operations of nature; for horns are not a production

duction from the cutis, and although not always formed upon a bony core, but frequently upon a ſoft pulp, that ſubſtance differs from common cutis in its appearance, and extends a conſiderable way into the horn: it is, therefore, he thinks, probable, that this pulp requires a particular proceſs for its formation. In ſupport of this opinion he gives the following fact:

"A ſheep, about four years old, had a large "horn, three feet long, growing upon its flank. "It had no connection with bone, and appear- "ed to be only attached to the external ſkin. "It dropped off in conſequence of its weight "having produced ulceration in the ſoft parts "to which it adhered. Upon examining it "there was a fleſhy ſubſtance, ſeven inches long, "of a fibrous texture, filling up its cavity upon "which the horn had been formed."

Towards the concluſion of his paper Mr. Home obſerves, that the caſes of horns, as they are commonly termed, upon the human head, are no more than cuticular productions ariſing from a cyſt, which in its nature is a variety of thoſe tumours deſcribed by Mr. Hunter under the general name of cuticular encyſted tumours.

The principle upon which the production of theſe excreſcences depends being once explain-

ed, the modes of preventing their formation, and removing them when formed, will, he thinks, be readily underſtood, the deſtruction of the cyſt being all that is required for that purpoſe. This, he obſerves, may be done before the tumour opens externally, or even after the excreſcence has begun to ſhoot out, and will be better effected by diſſection than eſcharotics, ſince the ſucceſs of the operation depends upon the whole of the bag being removed.

Theſe encyſted tumours, he obſerves, when conſidered as varieties of the ſame diſeaſe, form a very complete and beautiful ſeries of the different modes by which the powers of the animal œconomy produce a ſubſtitute for the common cuticle upon parts which have been ſo much affected by diſeaſe as to be unable to reſtore themſelves to a natural ſtate.

XIII. *Ex-*

XIII. *Experiments on Human Calculi. In a Letter from Mr.* Timothy Lane, *F. R. S. to* William Pitcairn, *M. D. F. R. S.*—Vide *Philoſophical Tranſactions of the Royal Society of London, Vol.* LXXXI. *for the Year* 1791. *Part* II. 4to. London, 1791.

THE lixivium ſaponarium of the late London Pharmacopœia, prepared with the addition of ſo much lime as nearly to free the ſalt of tartar of its fixed air *, having been uſed as a medicine for the ſtone and gravel, and its effects found very unequal, Mr. Lane was induced, twenty years ago, to examine different calculi, both as to the effect of the above lixivium and of fire upon them.

Great diſparity was obſerved, ſome being diſſolved, and others ſcarcely altered in their figure.

When tried by fire, ſome, we are told, were nearly evaporated by a red heat, while others retained their form.

Different parts even of the ſame calculus, he obſerves, varied conſiderably.

* See Letter to Dr. Heberden, Medical Tranſactions, Vol. I. p. 112.

For

For the ſake of greater accuracy the experiments were carefully repeated; and for this purpoſe fourteen ſpecimens were collected, ſome of which were of the ſame calculus, and others different calculi.

In the experiments by fire our author was favoured with the aſſiſtance of Mr. Staneſby Alchorne, of the Tower, to whom were ſent ten grains of each ſpecimen, in ſeparate papers, which were numbered.

The contents of each paper were placed in ſeparate cupels, under a muffle, the ſame as is uſed by him for aſſaying gold and ſilver. The fire was raiſed gradually till the furnace was fully heated: the time from raiſing the fire to the taking them out again was three hours, when it was concluded that whatever volatile matter they contained was expelled.

The ſame quantity, as above mentioned, of each ſpecimen being put into ſeparate numbered phials, with one ounce meaſure of the lixivium in each, continued forty-eight hours, the phials being frequently ſhaken to forward the ſolution.

The clear liquor of each phial was decanted into freſh phials, and a quarter of an ounce more lixivium was added to ſuch of the ſpeci-

mens as were undiſſolved; after twenty-four hours they were poured out of the phials into ſeparate filtering papers, each numbered, and the phials waſhed with diſtilled water, which was alſo poured into the papers, ſo that all that remained undiſſolved might be detained by the papers, which, with their contents, were carefully dried.

The following table ſhows the remains of each:

	Unſublimed.	Undiſſolved.
	Grains.	Grains.
N° 1.	$1\frac{1}{2}$	$\frac{3}{4}$
2.	$2\frac{1}{2}$	2
3.	$\frac{1}{4}$	$\frac{1}{4}$
4.	$1\frac{1}{4}$	2
5.	$\frac{1}{4}$	0
6.	$3\frac{1}{2}$	$2\frac{1}{2}$
7.	$3\frac{1}{4}$	6
8.	6	$8\frac{1}{4}$
9.	$6\frac{1}{4}$	$6\frac{1}{4}$
10.	$6\frac{1}{4}$	$7\frac{1}{2}$
11.	$\frac{1}{4}$	$\frac{1}{2}$
12.	$\frac{1}{4}$	0
13.	$5\frac{1}{2}$	4
14.	6	$5\frac{1}{4}$.

The

The appearances of each after calcination were as follows :

N° 1, 3, 7, 8, left a fine white and ſoft powder.

N° 4, 5, 11, 12, left a white and gritty powder.

N° 2, 6, 9, 10, 14, were partly in powder white and gritty, with ſome lumps of a dark colour, as if not fully calcined.

N° 13. Of this the figure, it is obſerved, was not greatly altered ; it remained hard, and part of it appeared as if inclined to flux.

After being in the lixivium forty-eight hours,

N° 8, 9, 13, 14, were found ſoft.

N° 7 and 10 remained hard.

Theſe ſix, Mr. Lane obſerves, were ſeparately taken out of the lixivium and put into a mortar, and rubbed or broken, and then carefully returned to their ſeparate phials before the ſecond addition of lixivium, in order to forward the ſolution.

Mr. Lane gives the following deſcription of the different ſpecimens :

N° 1 was the external part of a laminated calculus, of a light yellowiſh brown colour.

The

The nucleus, ſo called, as being the central part, was, he obſerves, of a much deeper colour, and had been found not ſo ſoluble in lixivium as the light-brown part.

N° 2 was the external part of a calculus, in colour like dirty tobacco-pipe clay.

The nucleus of this, we are told, was of a bright yellow, and more ſoluble in lixivium than the whitiſh part.

N° 3 was a light-brown laminated calculus.

N° 4 and 5 were two ſpecimens from one calculus; of which N° 4 was the external coat, of a dirty tobacco-pipe-clay colour.

N° 5 was the internal part of N° 4, yellowiſh like N° 1.

N° 6 was a calculus taken out of the urethra; of a greyiſh white, inclining to yellow, and of a porous texture.

N° 7 was a calculus about the ſize of a nutmeg, taken from a child of a year old; it was aſh-coloured, in waves of different ſhades, laminated and hard.

N° 8 was a dark-brown and very hard calculus, of the mulberry kind.

N° 9 and 10 were two ſpecimens from one calculus; of which N° 9 was the external whitiſh part,

part, which appeared like a coat of calcareous earth, covering an irregular mulberry calculus.

The covering of this calculus induced our author to ſuſpect that lime or lime-water might have been taken, and, by being decompounded by freſh urine, containing fixed air, have formed this covering. Other calculi, he obſerves, have afforded the ſame ſuſpicions.

This circumſtance leads him to ſuggeſt that, in future, an account of medicines taken might, in theſe caſes, afford much information, when joined with the examination of different parts of large calculi taken out of the bladder.

N° 10 was the brown mulberry part covered by N° 9. The three following were parts of one large, laminated calculus; of which

N° 11 was the external lamina, of a brownish yellow.

N° 12 was the central part, called the nucleus, of a pale orange colour.

N° 13 conſiſted of ſome of the laminæ, between the nucleus and the external coat, of a ſparkling appearance.

N° 14 was a whitiſh, porous, and eaſily broken calculus.

The experiments by fire explain, Mr. Lane thinks,

thinks, the different accounts of authors, reſpecting the component parts of calculi.

He obſerves that, in general, thoſe which contained the largeſt proportion of volatile parts were moſt ſoluble in lixivium.

The inſolubility of ſome of them explains, he thinks, the want of ſucceſs in ſeveral caſes, where lixivium, ſoap, and lime-water, have been given as remedies.

The ſolubility of others, however, joined with the teſtimony of reputable authors, and his own experience for near thirty years, confirm, in his opinion, the ſalutary effects of lixivium in many caſes.

It frequently happens, he obſerves, that, in fits of the gravel and ſtone, gravel or ſmall pieces of calculi are diſcharged, which ſhould be examined.

If they are found to be perfectly ſoluble in lixivium *(aq. kali puri)*, the remedy, he remarks, will be obvious; if imperfectly, doubtful; if inſoluble, lixivium will only irritate, without benefit.

XIV. *Ex-*

XIV. *Experiments and Obſervations to inveſtigate the Compoſition of James's Powder.* *By* George Pearſon, *M. D. F. R. S.*—Vide *Philoſophical Tranſactions of the Royal Society of London, Vol.* LXXXI. *for the Year* 1791. *Part* II. 4to. London, 1791.

ALTHOUGH the powder, which is the ſubject of the paper before us, has for more than thirty years been very extenſively uſed in the cure of continued fevers, the Public have not been accurately informed of the particular nature of this ſubſtance. It was originally a patent medicine; but it is well known that it cannot be prepared by following the directions of the ſpecification in the Court of Chancery. Dr. Pearſon has, therefore, been induced to make it the ſubject of chemical inveſtigation, and by a great number of judicious experiments, ſynthetical as well as analytical, has attempted to aſcertain the nature and manner of preparing this medicine.

He begins with deſcribing its ſenſible properties. Some parcels of this preparation, he obſerves, are white, but in general it has a yellowiſh caſt; and this ſhade is more evident in

ſome ſpecimens than in others. It is ſaid, that this powder cannot, in general, be made at different times of preciſely the ſame ſhade of yellow or degree of whiteneſs. Sometimes with the aid of a lens a few very ſmall ſhining *ſpicula* are ſeen mixed with powder. When preſſed between the fingers it feels ſmooth, with ſome rather rough particles, and it is gritty in the mouth. Moſt parcels at firſt are taſteleſs; but in about a minute there is a ſlight braſſy taſte. It is perfectly inodorous.

Dr. Pearſon next ſpeaks of its ſpecific gravity. He obſerves that it feels much heavier than any of the common earths and ſtones in a pulverized ſtate. One of the phials, nearly full, in which it is ſold, reckoned to hold a quantity equal to twelve packets, or 480 grains, contained 470 grains troy weight of James's Powder. This phial, filled with diſtilled water to the ſame height that it had been by the powder, was found to contain nearly four drachm-meaſures, or about 240 grains, of this liquid.

In another experiment made in a different manner, for the purpoſe of aſcertaining the ſpecific gravity of this powder, the quantity which nearly filled a phial weighed 437 grains; and upon filling the ſame phial, to the ſame height,

with diſtilled water, the temperature of which was 65°, the water weighed 250,2 grains. The reaſon of the variation in theſe reſults, in making uſe of different parcels of this medicine, our author thinks, will be obvious, from the account he gives of its preparation, and the great difficulty of determining, with accuracy, the ſpecific gravity of powders.

After deſcribing the effects of fire on this ſubſtance, and obſerving that the experiments made for this purpoſe indicated the preſence of a metallic calx, a part at leaſt of which was that of antimony, mixed with earthy matter, he proceeds to experiments with different menſtrua, viz. water, the acetous, nitrous, and marine acids.

From his experiments on James's powder with the firſt of theſe menſtrua, viz. water, he thinks we may conclude,

1. That the whole, or a part of it, is ſoluble, or at leaſt may be ſuſpended, in about 2000 times its weight of pure water, cold; and in about half this quantity of boiling water.

2. That this ſolution contains calcareous earth united to an acid, or ſome other ſubſtance, from which it cannot be diſunited by cauſtic or mild fixed alkalies.

3. That

3. That this ſolution contains a metallic calx, a part of which at leaſt is that of antimony uncombined, or at leaſt not united to any acid with which it forms a compound ſoluble in water.

4. That the ſubſtance in a nitrous ſolution of the part of James's Powder that had been diſſolved in water, which was obſerved to precipitate lime from lime-water, and which percipitate was not ſoluble in a large quantity of vinegar, is, probably, phoſphoric acid from phoſphorated lime decompoſed by nitrous acid.

The precipitation by muriated barytes and nitrated ſilver, which is deſcribed as having taken place in two of the experiments, could not, our author thinks, be from vitriolic and marine acids conſiſtently with the preceding experiments; and he could not have conjectured what was the ingredient in James's Powder which occaſions it, if he had not found, that muriated barytes is not only a teſt of vitriolic but of phoſphoric acid united to lime and alkalies; and that the acid of phoſphorus will alſo produce a turbid appearance with nitrated ſilver.

The preſence of calx of iron was detected in theſe experiments, but in ſo ſmall a quantity that our author is inclined to conſider it only as an accidental ſubſtance.

His experiments with the acetous acid indicate the ſame kind of ſubſtances as the experiments with water, namely, calcareous earth in a combined ſtate; phoſphoric acid ; calx of antimony, and calx of iron. It appears alſo from theſe experiments that James's Powder is either wholly or partially ſoluble in about 300 times its weight of concentrated acetous acid.

It appears from the experiments with nitrous acid, that this menſtruum, by two affuſions, in a large proportion, aided by trituration, digeſtion, and heat, diſſolved $\frac{108}{240}$ of James's powder that had been expoſed to the action of water and acetous acid ; but our author obſerves, that from the ſmallneſs of the quantity which was contained in the nitrous acid the ſecond time it was applied, and from its being principally calcined antimony, not more than two of the ſix grains afforded by the ſolution, perhaps, ſhould be conſidered as *diſſolved*, for the reſt may be ſuppoſed to have been merely *ſuſpended*.

The firſt ſolution alſo in this menſtruum, it ſeems, was not filtered, and the acid was conſiderably redundant, and there were found in it ſeveral grains of calcined antimony. The real quantity *diſſolved* might, therefore, our author thinks, probably be eight grains leſs than the

above 108 ſtated. According to this mode of calculation, the proportion of the ſoluble part of James's Powder in nitrous acid is $\frac{100}{240}$, or about $\frac{41}{100}$.

2. The whole of this ſoluble part, except a little calx of antimony, he thinks, is deciſively phoſphoric acid and calcareous earth: which two ſubſtances, he obſerves, may reaſonably be ſuppoſed, from theſe experiments, to have been united together, and to have been in the ſtate of phoſphorated lime in this powder. Conſequently the proportion of this phoſphorated lime, conſidered as the ſoluble part of James's Powder in the experiments in queſtion with nitrous acid, appears to be 40 *per cent.* making a deduction of 1 *per cent.* for the antimonial calx contained in the nitrous acid, in theſe experiments. He ſuſpects, however, that the powder which reſiſted ſolution in this menſtruum may contain more phoſphorated lime; and this conſideration prevents him aſſigning at preſent the above 40 *per cent.* as the whole quantity of it in James's Powder. It cannot, however, he thinks, be a ſmaller proportion.

He does not reckon the calx of iron in theſe calculations, becauſe it is in too ſmall a quantity, and is apparently only to be looked upon as an accidental extraneous ſubſtance. He ſuppoſes

too, that the water and acetous acid applied to the James's Powder uſed in theſe experiments, carried off a proportion of its ingredients equal to that in the remaining powder.

From the experiments the author next relates, we learn, that by repeatedly digeſting and boiling in marine acid, and in *aqua regia*, that part of James's Powder which reſiſted ſolution in nitrous acid, which was $\frac{108}{240}$, 77 grains were carried off by theſe menſtrua; but conſidering the ſmall proportion contained in theſe acids after the two firſt affuſions, which afforded 57,15 grains, and ſuppoſing the calx to be neither increaſed nor diminiſhed in weight by the acids, the real quantity of ſoluble and fuſible calx of antimony, he thinks, may be ſtated to be that of the Algaroth powder which he obtained in theſe experiments; for the other kind of antimonial calx procured by ſubſequent affuſions was probably only *ſuſpended*. It appears, therefore, that 240 grains of James's Powder, afforded, by theſe experiments with marine acid, 57,15 grains of Algaroth powder, and 19,85 grains of a leſs ſoluble and more difficultly fuſible calx of antimony, with a ſmall proportion of phoſphorated lime. The reſiduum, amounting to 55 grains, was next examined; from his experiments,

ments, however, on this insoluble matter, he could only conclude that it contained calx of antimony; but as to the proportion of this calx, and the other substance with which it is joined, he conjectures that it may be about half the quantity of the insoluble powder; and that the other half is antimonial calx and phosphorated lime, so highly calcined and vitrified together as to resist solution in acid menstrua, decomposition by charcoal, and fusion with fixed alkalies, but not by phosphoric acid.

Our author observes, that he should not have been satisfied with here terminating this analysis without enquiring further into the nature of this insoluble matter; but he discontinued this analytic investigation in order to derive light from synthetic experiments.

His experiments on this insoluble matter seem to show, that the proportion of antimonial calx is not so great as might have been assigned from the experiments with nitrous acid, marine acid, and *aqua regia*.

The substances and proportions of them, obtained from 240 grains of James's Powder, were as follow:

	Grains.
Phoſphorated lime, with a little antimonial calx, - -	100,
Algaroth powder, - -	57,15
Inſoluble antimonial calx, with a little phoſphorated lime, - -	19,85
The ſame inſoluble calx, with, probably, a little phoſporated lime, -	55,
Waſte, - - -	8,
	240,0

As it might be objected, that concluſions drawn concerning the nature of calces might be erroneous if nitrous acid had been applied previouſly to ſubſtances containing them, our author made an experiment with marine acid applied to James's Powder, which had not been expoſed to the action of nitrous acid, or any other menſtruum. From this experiment he thinks it may, perhaps, be fairly concluded that the properties of the calx in James's Powder are not altered by nitrous acid to affect its ſolubility in marine acid.

To know whether James's Powder contained any ſubſtance that could be decompoſed by mild fixed alkalies, 100 grains of James's Powder were boiled in ſix ounces of water, with fifty

grains

grains of mild alkali of tartar, for three hours, and then the remaining liquid was filtered, and evaporated to dryneſs; but the matter left after evaporation was nothing but the alkali uſed in the experiment, with a little of the powder itſelf.

The reſult, we are told, was the ſame on making the experiment with cryſtallized mineral alkali inſtead of alkali of tartar.

Although the inability to prepare James's Powder would not prove the above concluſions, with reſpect to its compoſition, to be erroneous, the being able to compoſe a ſubſtance poſſeſſing all the ſame properties as James's Powder, by uniting or mixing together the ſubſtances ſhewn by the preſent analyſis to enter into its compoſition, would afford, our author thinks, all the proof and demonſtration which can be had from chemiſtry.

His analyſis, he obſerves, ſhewed no eſſential ingredients of James's Powder but antimonial calces, phoſphoric acid, and calcareous earth, which two laſt ſubſtances appeared to be united together; but it would, he contends, have been vain and unneceſſary labour to have attempted to make this powder by mixtures of any of the commonly known calces of antimony and phoſphorated

phorated lime; becauſe none of them, from their well-known qualities, could form a powder of the ſame colour and ſpecific gravity as James's Powder, and like it partially ſoluble in acids. From his experiments, however, the probability, he obſerves, was evident, that this ſubſtance might be made by calcining together antimony and bone-aſhes; which operation produces a powder called Lile's and Schawanberg's fever-powder; a preparation diſcribed by Schröder * and other chemiſts more than a century ago. The receipts for this preparation differ in the proportion of the antimony to the bone aſhes, and in the ſtate of the bone; ſome directing bone ſhavings to be previouſly boiled in water; others ordering them to be burnt to aſhes before calcining them with antimony; while in other preſcriptions the bone ſhavings are directed to be burnt with the antimony. According to a receipt in the poſſeſſion of Mr. Bromfeild, by which this powder was prepared forty five years ago, and before any medicine was known by the name of James's Powder, two pounds of hart's-horn ſhavings muſt be boiled to diſſolve

* Pharmacop. Med. Chymic. 8vo. Lug. Bat. 1672, p. 428.

all

all the mucilage, and then, being dried, be calcined with one pound of crude antimony, till the ſmell of ſulphur ceaſes, and a light grey powder is produced. The ſame preſcription, we are told, was given to Mr. Willis, about forty years ago, by Dr. John Eaton, with the material addition, however, of ordering the calcined mixture to be expoſed to a great heat in a cloſe veſſel to render it white. Mr. Turner, it ſeems, made this powder above thirty years ago by calcining together equal weights of burnt hart's horn and antimony in an open veſſel, till all the ſulphur was driven off, and the mixture was of a light grey colour. He likewiſe was acquainted with the fact, that by a ſufficient degree of fire in a cloſe veſſel this cineritious powder turned *white* *. Mr. Turner alſo prepared this powder with a pound and a half of hart's-horn ſhavings

* Dr. Pearſon thinks it probable that this powder was made for ſeveral years with merely the heat neceſſary to carry off the ſulphur and calcine the bone, in an open veſſel over a charcoal fire in a common grate, and conſequently it was of a light clay or aſh colour. In this manner, Mr. Bromfeild told him, he prepared Schawanberg's Powder 46 or 47 years ago. Its property of turning *white* in a greater degree of fire appears to have been a ſubſequent diſcovery.

and

and a pound of antimony, as well as with ſmaller proportions of bone. Schröder preſcribes equal weights of antimony and calcined hart's horn; and Poterius and Michaelis, as quoted by Frederic Hoffman, merely order the calcination of theſe two ſubſtances together (aſſigning no proportion), in a reverberatory fire for ſeveral days. In the London Pharmacopœia of 1788, this powder is called *Pulvis antimonialis*; and it is directed to be prepared by calcining together equal weights of hart's-horn ſhavings and antimony.

Our author obſerves that powders made from various proportions of antimony and bone aſhes, after ſolution in nitrous acid, left a reſiduum of antimonial calx much leſs or greater in quantity than James's Powder did by the ſame menſtruum, except two of Mr. Turner's proportions, viz. two parts of antimony and one of calcined bone, and equal weights of bone ſhavings and antimony. The quantity of this calx was, however, greater, it ſeems, in the powder from the former of theſe two laſt proportions than the latter of them; which latter correſponded ſometimes exactly, and always nearly, with the weight of the calx from a given weight of James's Powder. This calx afforded alſo,

we

we are told, the ſame proportion of Algaroth powder as the calx in James's Powder; and the inſoluble part of the calx afforded metallic grains like thoſe from the inſoluble part of the calx in that powder.

Dr. Pearſon found then an exact correſpondence between what he conſiders to be the eſſential and peculiar properties of James's Powder, and the properties of a powder made by uniting or mixing together the ingredients of James's Powder found by analyſis. But, in order to ſhow the identity or difference of the qualities of theſe two ſubſtances, he made comparative obſervations on them, and repeated his analytic experiments on James's Powder with the preparation made by calcining together equal weights of bone ſhavings and antimony, in an open veſſel, to carry off the ſulphur, and then in cloſe veſſels, applying ſuch a degree of fire as to render them white, that is, on the ſame preparation as the *Pulvis antimonialis* of the London Pharmacopœia.

Firſt, he compared, more particularly, the ſenſible qualities of ſeveral different ſpecimens of James's Powder with various parcels of the *Pulvis antimonialis* made by different chemiſts. All of theſe, he obſerves, would be called white powders,

powders, but not two of them were ſo in the ſame degree. Moſt of the papers of the *Pulvis antimonialis* were whiter than thoſe of James's Powder; but others were of a very light ſtone colour, and ſome had a ſhade of yellow, ſo as to reſemble very exactly James's Powder; but all the parcels of James's Powder had either a ſhade of yellow or of ſtone colour, and none were perfectly white, or ſo white as ſome ſpecimens of the *Pulvis antimonialis*. Some of the parcels of James's Powder and of the *Pulvis antimonialis* taſted braſſy; and other ſpecimens of both powders had no taſte. All of theſe powders were gritty. Moſt of the parcels of the *Pulvis antimonialis* were a little ſpecifically heavier than thoſe of James's Powder. The ſpecific gravity of both powders was increaſed by expoſing them to ſuch a degree of fire as brought them into almoſt a ſemi-vitrified ſtate; and on the contrary, the ſpecific gravity of the *Pulvis antimonialis* was leſs than it is in its uſual ſtate, when made in ſuch a degree of fire that the mixture preſerves the powdery form.

The experiments with water on the *Pulvis antimonialis* produced, we are told, the ſame kind of appearances, but more ſlightly than thoſe with James's Powder; for the hot ſolution of

the former grew leſs milky on cooling than that of the latter, and on evaporation to dryneſs leſs ſediment was found of the ſolution of *Pulvis antimonialis* than after that of James's Powder.

The experiments with acetous acid on the *Pulvis antimonialis* ſhewed that this menſtruum diſſolved ſometimes a greater, and ſometimes a ſmaller proportion of it than of James's Powder; and the diſſolved matter was found to be antimonial calx, phoſphorated lime, and calx of iron, and no other ſubſtance.

The proportion of ſoluble matter in nitrous acid was the ſame, it is obſerved, or nearly ſo, of the *Pulvis antimonialis* as that of James's Powder; and this diſſolved matter was phoſphoric acid, calcareous earth, with a little antimonial calx, and a minute portion of calx of iron, as exactly as could be expected from the nature of the ſubſtances and the experiments, in the ſame proportion as thoſe in James's Powder.

The Algaroth powder, which our author obtained by means of ſolution of the *Pulvis antimonialis* in marine acid, was in the ſame proportion, as nearly as could reaſonably be expected from the nature of the experiments, as that obtained from James's Powder. And the part that reſiſted

reſiſted ſolution in this menſtruum was partially reducible to a metallic form, and had otherwiſe the ſame properties, as far as diſcovered, as the inſoluble part of James Powder.

Dr. Pearſon, having now formed a powder poſſeſſed of properties ſimilar in kind to every one of thoſe aſcertained in James's Powder, with ſcarcely any difference in the degree of them, contends, that, if it be thought that among theſe properties are thoſe which are eſſential and peculiar ones of James's Powder, the concluſion that theſe two are the ſame kind of things muſt be admitted to be juſt. The nature of one of the ingredients of James's Powder, viz. the irreducible part of the inſoluble matter, he confeſſes, is not fully elucidated by his ſynthetic experiments; but in ſo far as they ſhow, that this part equally exiſts in the powder formed by calcining together antimony and bone, which is concluded to be James's Powder, the objection againſt the concluſion with reſpect to the identity of the two ſubſtances, on the ground of this inconſiderable part of James's Powder not being well underſtood, muſt, he thinks, be of little weight.

We come now to the ſynthetic experiments which our author made with the view of confirming

firming or invalidating the concluſions drawn from the above analyſis, with reſpect to the ingredients and proportions of them in James's Powder.

From one of theſe experiments (the 3d), which conſiſted in calcining together equal weights of antimony and hart's-horn ſhavings, it appears that the calcination of antimony with bone aſhes is much more ſpeedy than when by itſelf; and from another of them (the 5th) we learn what degree of fire is neceſſary to render the antimony calcined with bone of a white colour; and that this whiteneſs does not depend on the air, but on the fire. We ſhall here give the whole of this experiment.

" *(a)* 1500 grains of the calcined mixture " of antimony and bone, Exp. 3. were kept " red hot in a cloſe veſſel for half an hour. On " cooling, I found the powder changed from " a cineritious or clay colour to a whitiſh colour " with a ſhade of yellow. The ſides of the " crucible were not glazed. The pyrometer in " the middle of the powder had contracted to " 40°. This powder was much inferior in " whiteneſs to James's Powder, being much " yellower.

" *(b)* Another parcel of the ſame powder, " Exp. 3. was expoſed in the ſame manner,

" but to a greater degree of fire, in which the " crucible was almoſt white hot for half an hour. " After cooling, the powder was found changed " to a looſely cohering, ſnow-white, heavy maſs, " and the ſides of the crucible were covered " with a yellow glaze. This maſs, which was " eaſily detached from the veſſel, was found " covered with a yellow vitreous coat over the " whole furface of it that had been in contact " with the crucible. In the white ſolid, on " breaking it, many argentine *ſpicula* were ſeen. " The pyrometer uſed in all theſe experiments " indicated 71°.

" *(c)* 1500 grains of the ſame parcel, Exp. 3. " were expoſed in an open crucible to the fire " of a melting furnace; no fumes aroſe till the " crucible began to be almoſt white hot. Af- " ter inverting another crucible, with a ſmall " hole in its bottom, the fumes continued to " aſcend at times through the aperture for a " quarter of an hour. The crucible was then " taken out of the fire, and on cooling a whitiſh " powder was found, but no glazing, and the " pyrometer indicated 28°. On again expoſ- " ing this crucible with one inverted over it in " the melting furnace, but to a greater degree " of fire, ſtill more fumes aroſe; but, on cool- " ing, the charge was ſtill in the ſtate of a pow-

" der,

" der, though whiter than before; and the in-
" side of the inverted crucible was covered with
" silvery particles, and the hole of it was sur-
" rounded with argentine *spicula*, in a stellated
" form. The pyrometer indicated 39°. On
" reducing a little of this powder to a greater
" degree of fineness, it was as white as James's
" powder, with a yellowish cast like it, but in-
" ferior in whiteness to a specimen of *pulvis*
" *antimonialis*. This crucible, containing its
" charge, with a cover closely luted on it, was
" put again into the fire, which was raised much
" higher than before; and, after being exposed
" in it twenty minutes, the powder in the cruci-
" ble became a loosely cohering solid, as white
" as snow, with a vitreous yellow coat, as be-
" fore observed; the inside of the crucible was
" glazed and covered with *spicula*. The pyro-
" meter-piece in the middle of the powder was
" also covered with a yellow coat, but not glaz-
" ed, and it indicated 81°. This loosely coher-
" ing solid, being pulverized, afforded a *whiter*
" powder than James's powder.

(d) The crucible, with its charge *(b)*, hav-
" ing a cover well luted on it, was again put
" into the furnace, and the fire raised to almost
" as great a degree as I was able. This intense

" heat was kept up above an hour. After cool-
" ing, a white hard ſolid maſs was found with-
" in the crucible. On breaking the veſſel, to
" detach from it the charge, this ſolid maſs was
" found as hard as marble, and to have received
" its figure from the crucible. Its ſurface was
" covered with a yellow vitreous coat, and the
" whole inſide of the veſſel had a beautiful
" gold-coloured glaze with many argentine
" *ſpicula*. The pyrometer piece in the middle
" of the charge was alſo covered with a fine
" yellow glaze, and indicated 166°. This
" ſolid, hard maſs weighed only twenty-one
" grains leſs than before the experiment,
" though the whole inſide of the crucible was
" glazed, and had ſhining *ſpicula* upon it. A
" piece of this hard maſs being pulverized, it
" afforded a whiter powder than James's pow-
" der is in general."

Our author thinks it will not be difficult, from his experiments, to give a probable reaſon for the James's powder being generally of a yellow-iſh caſt, and for different parcels of it, as well as of the *pulvis antimonialis*, being generally of different degrees of whiteneſs and ſhades of yellow. The colour of this preparation is, however, he obſerves, a very delicate one. He

once

once directed a perſon to calcine together antimony and bone ſhavings, in the uſual manner, to that ſtate in which the white powder may be produced by a due degree of fire; but, inſtead of a ſnow-white maſs, he could not by any degree of fire obtain any colour but a dirty whitiſh or light ſtone colour; though repeated calcinations were employed. The reaſon of the failure was, that the earthen diſh had been broken during the calcination, and a few very ſmall pieces of it had ſcaled off, and being mixed with the powder occaſioned this diſappointment with reſpect to colour. The ſame diſappointment, it ſeems, has been alſo occaſioned by uſing a ruſty iron rod in calcining the mixture.

The bone-aſhes procured from the ſal ammoniac and ſpirit of hart's horn manufactories, frequently failed, he tells us, in producing a white powder; and ſo did ſometimes the bone-aſhes, called prepared hart's horn, ſold by druggiſts. Even after a fine white-coloured maſs had been made, if it was pulverized in an iron mortar that had extremely little calx upon its ſurface, or dirt, the powder was not white.

Dr. Pearſon has been told by ſome of the perſons who prepare the *pulvis antimonialis*, that the whiteſt colour is obtained by firſt boil-

ing the bone ſhavings to diſſolve their mucilage, and then calcining them with antimony as above ſhewn. Mr. Lile's receipt, he obſerves, directs previous decoction of the hart's horn.

He thinks that the yellow coat and glaze on the ſides of the crucible and ſurface of the calcined mixture of bone and antimony, which, he obſerved in his experiments, ſhould be aſcribed rather to the fuſion of the clay of the crucible with the antimonial calx, than to the greater degree of fire in the part of the crucible in which it takes place; or than to the calx of iron and ſiliceous earth of the veſſel; becauſe the ſame yellow coat and glazing are produced on the Wedgwood pyrometer pieces, which are placed in the middle of the charge, and where the degree of heat cannot be ſo great as nearer the ſide of the crucible, and yet a ſnow-white maſs is produced between theſe clay pieces and the ſides of the crucible. This yellow coat, he obſerves, is one reaſon for the powder being of a ſhade of yellow in ſome ſpecimens.

From an experiment which he relates, he thinks it very probable, that no degree or duration of fire, applied in open or cloſe veſſels to antimony alone, can produce a calx of the ſame kind as that in James's powder: nor, perhaps,

that ſuch a powder can be compoſed by fire applied, in cloſe veſſels, to calx of antimony mixed with calcined bone; but if antimony duly calcined be mixed with calcined bone, and expoſed to air, in a due degree of fire, for a ſufficient length of time, and then a ſtill greater degree of fire be applied to it in cloſe veſſels, ſuch a compound, he obſerves, may be formed as James's powder. The ſame experiment, he thinks, alſo proves, that the ſulphur in antimony is no ways neceſſary to the formation of this compound.

Dr. Pearſon concludes his paper with obſerving that, from the whole of his analytical experiments, it appears:

1. That James's powder conſiſts of phoſphoric acid, lime, and antimonial calx; with a minute quantity of calx of iron, which is to be conſidered as an accidental ſubſtance:

2. That either, theſe three eſſential ingredients are united with each other, forming a triple compound; or, phoſphorated lime is combined with the antimonial calx, compoſing a double compound in the proportion of about fifty-ſeven parts of calx and forty-three parts of phoſphorated lime:

3. That this antimonial calx is different from any other known calx of antimony in ſeveral of its chemical qualities. About three fourths of it are ſoluble in marine acid, and afford Algaroth powder; and the remainder is not ſoluble in this menſtruum, and is apparently vitrified:

And from the ſynthetic experiments he has related, he thinks, it appears, that by calcining together bone-aſhes, that is, phoſphorated lime, and antimony in a certain proportion, and afterwards expoſing the mixture to a white heat, a compound was formed conſiſting of antimonial calx and phoſphorated lime, in the ſame proportion, and poſſeſſing the ſame kind of chemical properties, as James's powder.

Beſides the preparation, which is more immediately the ſubject of this paper, Dr. Pearſon has examined another medicine ſold under the title of "James's powder for horſes, horned cattle, hounds, &c." It is a light clay-coloured, gritty, taſteleſs ſubſtance, in which are ſeen ſmall *ſpicula*; and it appears to him to be nothing more than James's powder for fevers, or Lile's powder, made by calcining antimony and bone-aſhes together in open veſſels.

XV. *Ac-*

XV. *Account of a Case of double Hare Lip, accompanied with a Fissure of the Palate; with Remarks. By M.* Chorin, *one of the Surgeons of the Hotel Dieu at Paris.*—Vide *Journal de Chirurgie*, Tom I. 8vo. Paris, 1791.

THE patient, whose case is here related, was a healthy girl, five years old, who was admitted into the Hotel Dieu at Paris, on the 7th of September, 1790, for the cure of a hare lip, the appearances of which are represented in fig. 1. of the annexed plate. In the upper lip, under the nostrils, there were two fissures, one third of an inch in width, which extended into the *fossæ nasales*, and were separated from each other by a small roundish protuberance (b. fig. 1.) connected with the nose, and shorter than either of the two portions of the divided lip. Behind this protuberance was a portion of the upper jaw, half an inch wide, which projected more forwards than the rest of the maxillary bones, from which it was separated on each side by a fissure of about a quarter of an inch wide. This bony projection, which at its lower part was on a level with the alveolar process, supported the two middle incisors, (which were smaller

ſmaller than uſual, and moveable in their ſockets) and at its upper part correſponded with the *ſeptum narium*, near the lower edge of which was a fiſſure, three quarters of an inch wide, which divided the roof of the mouth and the *velum palati* from before backwards.

The patient could take hold of her food only with the canine teeth and the bicuſpides, ſo that ſhe chewed it with difficulty, and when ſhe attempted to ſwallow, a part of the aliment was forced into the *foſſæ naſales*, and another part came out through the fiſſures in the lips. Experience, however, it ſeems, had taught her to leſſen, in ſome degree, theſe inconveniences, by taking into her mouth only a very little food at a time; and M. Chorin remarks, that ſhe could ſwallow liquids with much leſs difficulty, as by inclining her head a little backwards ſhe was able to pour them, in ſome meaſure, immediately into the pharynx.

With reſpect to her powers of ſpeech, it is obſerved that all the ſounds ſhe uttered were naſal, or, as is commonly ſaid, through the noſe; and that ſhe was able to pronounce vowels pretty diſtinctly, but that her articulation of conſonants was ſuch, that ſhe could be underſtood

derſtood only by perſons who were much accuſtomed to her.

In order to bring the protuberance already deſcribed to a level with the lip, and to depreſs the projecting portion of the maxillary bones, M. Deſault, who, as the principal ſurgeon of the Hotel Dieu, undertook the treatment of the caſe, had recourſe to a linen bandage, which paſſed over the upper lip and was fixed at the back part of the neck. The good effects of this bandage in compreſſing the parts in queſtion were ſo obvious, that its uſe was continued till the 18th of September, when the operation was performed.

As the patient was in good health, no other preparation was deemed neceſſary than that of regulating her diet for ſome days previouſly to the operation. M. Chorin has thought it right to obſerve, however, that before this was performed care was taken that her hair ſhould be well combed, and likewiſe that mercurial ointment ſhould be applied to it, in order that, by preventing any uneaſineſs to the patient from vermin, the bandage might be more likely to remain the neceſſary time undiſturbed. At the ſame time lint was placed behind the ears, and in the cavities formed by the cartilages of the ears,

ears, that the preſſure of the bandage might be more conveniently ſupported by the patient.

During the time of the operation the patient was ſeated on a high chair, with her head againſt the breaſt of an aſſiſtant, whoſe hands, applied to the cheeks, puſhed the fiſſures of the lips forwards, and at the ſame time compreſſed the external maxillary arteries at the part where they paſs on the lower jaw. M. Deſault, who placed himſelf before the patient, and a little to the right, began the operation by taking hold of the edge of the left portion of the lip with the thumb and fore finger of his left hand, and then with a pair of ſciſſars, rounded on both ſides, and very ſharp, cut out the whole of the red part up to the openings in the noſe, perpendicularly to the thickneſs of the lip, taking care to remove a ſomewhat larger proportion of the lower part where the edge was rounded (c. fig. 1.). He next took hold of the lower part of the protuberance (b. fig. 1.), and, ſtretching it, cut away the left edge of it with the ſame precautions he had uſed with reſpect to the lip. After this he removed, in a ſimilar manner, the right edge of the protuberance and the portion of lip that correſponded with it. He now held the angle of the wound, correſponding

ing with the left fiſſure, between his thumb and fore finger, while he paſſed into the lip, at the diſtance of the twelfth of an inch from its looſe edge, and of a quarter of an inch from the wound, a gold pin (fig. VI.), beſpread with cerate, and, puſhing it backwards and a little upwards, brought it out in the fiſſure. The protuberance (b. fig. I.) being now ſo placed as to be on a level with the lip, the pin was puſhed on through this alſo, at about its middle, and from thence carried in the ſame manner through the right portion of the lip.

While M. Deſault was employed in bringing the parts together by holding the two ends of the pin, an aſſiſtant introduced behind the latter, and before the protuberance (b.) and the lip, a loop of waxed thread, which he drew downwards, in order to keep theſe parts on the ſtretch and in contact. Upon this loop the operator introduced another broader piece of waxed thread, which he carried ſeveral times croſs and round the pin, taking care to bring the thread ſometimes acroſs and at others under the protuberance (b.)

He next introduced a ſecond pin (fig V.) a quarter of an inch above the firſt, and paſſed it through the two portions of the lip and through

the intermediate protuberance with the ſame precautions he had uſed with the former one. He likewiſe twiſted a piece of waxed thread about this ſecond pin in the ſame manner as he had done about the other, and afterwards carried the thread alternately from one pin to the other, in the manner of what is called the twiſted ſuture, till he had covered the whole ſurface of the lip, (ſee fig. II.). The ends of the thread were then ſecured by a knot, and the loop which had ſerved to keep the parts in contact, and, on the ſtretch, was cut off as high as poſſible.

M. Deſault now placed on the cheeks two compreſſes (dd. dd. fig. III.), each of them an inch thick, and extending from the maſſeter muſcle to near the junction of the lips, and from the os malæ to the lower jaw. Theſe compreſſes were preſſed forwards, and ſupported in this poſition by an aſſiſtant. Small compreſſes were likewiſe placed between the extremities of the pins and the ſkin, and the lip was covered with a pledgit of lint, over which was laid a compreſs (cccc.) moiſtened with vegeto-mineral water.

The patient's head, previouſly to the operation, had been covered with a cotton cap, which

came

came down pretty low; over this a bandage, three ells long, and of the ſame breadth as the lip, was now carried from right to left ſeveral times round the head immediately above the eyebrows, and being faſtened by a pin behind the right ear, and on a level with the upper lip, was brought over the compreſs of the ſame ſide, and from thence under the noſe and over the compreſs of the left ſide to behind the left ear, where it was ſecured by another pin: the remainder of the bandage was carried round the head. In order to prevent the compreſſes and bandage, juſt now deſcribed, from being diſturbed, another ſmaller bandage (ii. ii.) was placed on each ſide. The middle of this paſſed obliquely under the chin, and one of the heads of it was brought over one of the compreſſes, while the other was carried behind the ear of the oppoſite ſide to the top of the head, where they were faſtened, care being taken to fix them to the compreſs on each ſide, and to the firſt bandage, by means of pins. The motion of the lower jaw was prevented by a ſling bandage (ff.), the heads of which croſſed each other at the back part of the head; and the whole was rendered ſtill more ſecure by ſeveral

turns

turns of a bandage (aa. aa.) carried round the head.

The operation, M. Chorin obſerves, was not long, nor the introduction of the pins very painful. The patient, who was put to bed immediately after it, ſlept during a part of the day, and the next morning ſhe complained of no pain, nor was any tenſion of the parts obſervable. The ſmall compreſs was then removed from the lip, and a freſh one, moiſtened, as before, with vegeto-mineral water, applied in its ſtead. On the third day the patient was allowed to eat ſome panada. On the fourth the pins were withdrawn by the point, their blunt ends having been previouſly cleaned, and ſmeared with cerate. On the fifth the threads fell off, and the reunion and conformation of the parts ſeemed to be perfect, (ſee fig. IV.); and the patient's pronunciation was obſerved to be much more eaſy. On the ſeventh the punctures occaſioned by the pins ſuppurated a little; but on the tenth they were cicatriſed, ſo that the marks of them could hardly be perceived. On the thirty-eighth the patient quitted the Hoſpital. M. Chorin adds, that he has ſince had occaſion to ſee her repeatedly, and has found that ſhe articulates diſtinctly; that the lip

lip is of its natural length; that the fiſſure of the palate is diminiſhed one third; and that the alveolar circle is regular.

M. Chorin is aware that ſeveral inſtances of deformity, nearly ſimilar to that which is the ſubject of the preſent caſe, are to be met with in books. The ancients, he obſerves, always conſidered the cure of ſuch caſes as abſolutely impoſſible; while, on the other hand, the moderns, from an opinion that the projection of the middle portion of the maxillary bones is to be conſidered as the greateſt obſtacle to the reunion of the lips, have adviſed it to be cut out: but experience, he contends, has ſhown that it is always eaſy either to bring together the lips over this bony projection, or to reduce the latter, by the preſſure of a bandage, to a level with the lateral parts of the jaw, ſo that its exciſion muſt conſequently be uſeleſs. Beſides, he obſerves, ſuch an operation is liable to many objections: it can hardly fail to occaſion inflammation of the neighbouring parts; it will leave a conſiderable ſpace between the maxillary bones; it will deprive the lip of its point of ſupport at the place where it is di-

vided; and if the reunion takes place, in ſpite of the diſadvantages of ſuch an arrangement, the action of the muſcles will ſoon leſſen the ſpace between the maxillary bones, and the upper jaw will become contracted enough to fall within the under one, a circumſtance which, at the ſame time that it renders maſtication very difficult, will occaſion a freſh deformity.

With reſpect to the operation of this ſpecies of hare lip, ſurgeons, he obſerves, are of different opinions concerning the manner of performing it, and the inſtrument fitteſt to be employed in it, as well as the means that are moſt likely to procure or facilitate the reunion of the parts. For while ſome have thought to render the operation more ſimple by reuniting one of the ſides of the lip to the middle part, and waiting till this ſhould be completely healed before they proceeded to operate on the other ſide; others have given the preference to the method adopted in the preſent caſe by M. Deſault, by performing the operation on both ſides at the ſame time.

Although Severinus, in his Treatiſe *de efficaci Medicina*, long ago recommended the uſe of the biſtoury in operations of this ſort, yet there are ſtill, our author obſerves, many ſurgeons who, in theſe caſes, give the preference

to

to the ſciſſars, and, in his opinion, not without reaſon; for with the ſciſſars the operation, he contends, is performed more ſpeedily and eaſily: in uſing them the ſurgeon is never under the neceſſity of ſeparating the lip from the gums, becauſe he does not cut upon paſteboard, as with the biſtoury, and he himſelf can hold the part that is to be cut: with the ſciſſars alſo the inciſion is more regular than with the biſtoury, the parts with the latter being almoſt always unequally divided; and experience has ſhown that the edges of the wound reunite with the ſame facility and quickneſs after an operation with the ſciſſars as when the knife has been employed.

The ſuture, M. Chorin obſerves, was for a long time ſuppoſed to be the only means of obtaining the reunion of a hare lip, and many ſurgeons, he adds, ſtill conſider it as the moſt certain and ſuitable method to be adopted in difficult caſes. The inconveniences which are ſometimes thought to be occaſioned by it oftentimes depend, he is convinced, on the manner of performing it, or on the dreſſings that are made uſe of. A great number of hare lips, ſucceſsfully treated by ſuture at the Hotel Dieu, might be brought, he tells us, in ſupport of

this aſſertion. Beſides, the bandage alone, however perfect it may be ſuppoſed to be, can never, he contends, keep the parts together ſo exactly and ſecurely as the ſuture; it cannot prevent the blood and the ſaliva from inſinuating themſelves between the edges of the wound; neither can it lengthen parts that are too ſhort, or aſſiſt in elevating ſuch as are too much depreſſed, advantages, he obſerves, which cannot be denied to the ſuture in the hands of a dexterous and experienced operator.

M. Chorin learns from Heiſter * that ſome German empirics were in the habit of uniting the parts in theſe caſes by ſtrong threads introduced at ſuitable intervals, in the manner of what is called the interrupted ſuture; but the inſufficiency of this method, our author obſerves, has long been known, and ſurgeons have adhered to the uſe of pins in theſe caſes, but have differed much about the compoſition and ſhape of theſe inſtruments. M. Chorin gives the preference to thoſe made of gold, becauſe they are not liable to ruſt, and it is poſſible to render their points as ſharp and nearly as cutting as thoſe of ſteel.

* Inſtitut. Chirurg. 4to. Amſtelædami, 1739, p. 674.

Speaking

Fig. I.

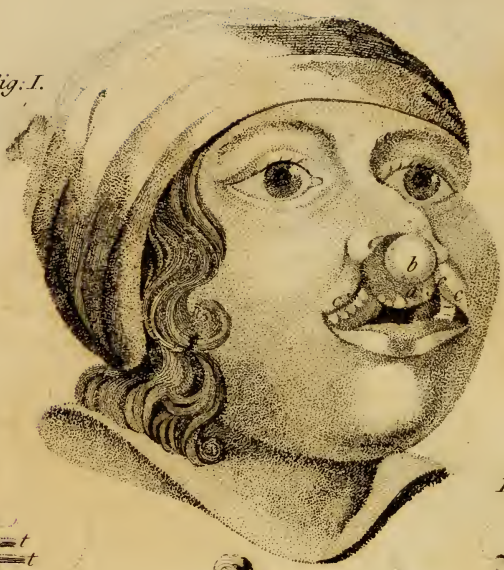

Fig. II.

Fig. IV.

Fig. III.

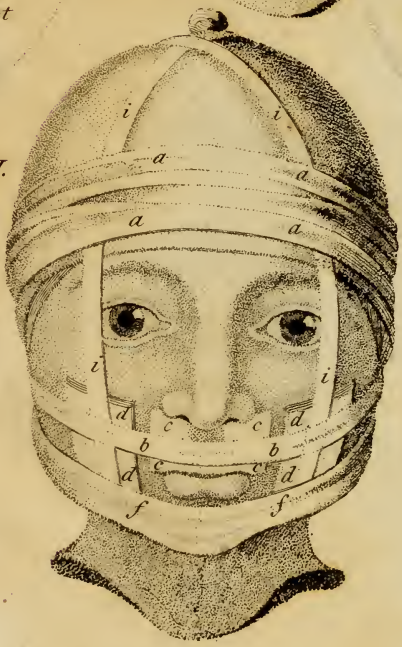

Fig. V.

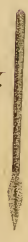

Fig. VI.

Fitzwilliam Sculp.

Speaking of the bandage employed in the caſe which is the ſubject of this paper, M. Chorin obſerves that it is more ſimple than any of the other uniting bandages which have been invented for the ſame purpoſe; that it acts ſolely on the compreſſes and cheeks; that it lies ſmooth and without preſſing on the lip, and of courſe does not endanger the cutting of the latter by the pins; and that if the compreſſes are properly preſſed forwards at the moment it is applied, it will be found to act in the ſame manner as another bandage will do, the heads of which are made to croſs each other under the noſe.

Explanation of Plate I.

Fig. I. State of the patient when admitted into the Hoſpital.

a. projecting portion of the jaw.

b. the protuberance between the two portions of the divided lip.

c. c. rounded angles of the diviſion of the lip.

Fig. II. The twiſted ſuture.

p. p. points of the pins.

t. t. their blunt ends.

Fig. III. Shows the apparatus employed in the caſe.

cccc. a ſmall compreſs placed on the wound.

dd. dd. thick compreſſes ſerving to preſs the cheeks forward.

bb. part of the uniting bandage paſſing over the compreſſes of the lips and cheeks.

ii. ii. bandages ſupporting the compreſſes of the cheeks.

ff. the ſling bandage.

aa. aa. turns of the roller fixing the whole of the apparatus.

Fig. IV. State of the lip after the cure.

Fig. V. and VI. Shape of the pins.

XVI. *An Account of a Child who drinks a great Quantity of Water. By M.* Vauquelin.—Vide *La Medecine Eclairée par les Sciences phyſiques, ou Journal des Découvertes relatives aux differentes Parties de l'Art de Guerir; redigé par M.* Fourcroy. *Tome* III. 8vo. Paris, 1792.

WE have here another inſtance of polydipſia in addition to the two we have already recorded *. The ſubject of the preſent caſe is a boy, five years old, who is ſaid to be of a lively diſpoſition, and (this preternatural thirſt excepted) apparently in good health. His pulſe, we are told, beats from eighty to eighty-five times in a minute; and he reſpires from fifteen to eighteen times within the ſame period.

In the courſe of twenty-four hours, during which our author remained with him in a room, the temperature of which was from 50° to 55° of Fahrenheit's thermometer, he drank ten quarts of water, at about 50°, and voided twelve quarts of urine. He commonly ſleeps ten hours of every twenty four. In the day time he generally requires a ſupply of drink

* Vol. II. page 73.

every half hour; and at night his ſleep is interrupted once, at leaſt, in every hour by his thirſt, and an inclination to make water; and it is obſerved that, notwithſtanding theſe frequent interruptions of his ſleep, he every night voids urine in bed.

When he drinks it is with evident marks of greedineſs; his eyes and countenance are expreſſive of the comfort he experiences; and the moment he has done drinking he appears lively and happy. If drink be at any time refuſed him, when his inclination for it returns, he becomes affected with a tremulous motion of the heart, which ceaſes the moment he has drank; and ſo great is his eagerneſs to allay his thirſt, that he ſeizes with avidity any thing within his reach that has the appearance of liquor, and, if not prevented, will even drink his urine. Soon after he has drank he has a ſenſation of coldneſs, with a ſlight ſhivering; his countenance, at the ſame time, acquiring a bluiſh tint, and his breath feeling cool.

At the time this account was drawn up the patient had laboured under this complaint four months. It was firſt obſerved a little before the period of his being ſeized with the ſmall pox.

The urine he voided, while M. Vauquelin was

was with him, was as clear as water, but had a disagreeable smell. It raised the mercury in the thermometer to 100°. It did not sensibly reddened tincture of tournesol; and was rendered but in a slight degree turbid by the addition of lime water. Volatile alkali produced no change in it. In its specific gravity it did not sensibly differ from water. On being exposed to the air it was observed to be much sooner decomposed than human urine, in a healthy state, usually is. This decomposition manifested itself by a milky colour, and by a very disagreeable smell. When exposed to heat, in an open vessel, it acquired a reddish colour in proportion as it evaporated, and its disagreeable smell was gradually dissipated. When about three fourths of it were evaporated it reddened tincture of tournesol; and, by completely evaporating it, our author obtained sixty-three grains of a residuum which contained phosphorated soda, volatile alkali, a large proportion of sea salt, a mucous extract, and phosphoric acid in an uncombined state.

We have seen that this child drank ten quarts of water, the temperature of which was about 50°, in twenty-four hours, and that in the same space of time he voided twelve quarts of urine at 100°. This great loss of the principle of heat

heat ſerves, in M. Vauquelin's opinion, to explain why the patient experiences coldneſs and ſhivering immediately after he has drank; and alſo why his breath is cold, and his face and lips appear of a violet colour.

As he voids by urine as much and even more than he drinks, and the diſcharge by the ſkin ſeems to be preternaturally diminiſhed, our author is diſpoſed to think that the exceſſive thirſt, in this caſe, may be occaſioned by ſome change that has taken place in the functions of the ſkin. This, he acknowledges, is merely an hypotheſis; but it is an hypotheſis, he obſerves, which would become realiſed, if, by reſtoring the perſpiration to its natural ſtate, the immoderate thirſt ſhould be relieved. He is aware, however, that ſeveral analogous facts muſt be collected, and compared with the preſent and the other few inſtances we have of this diſeaſe, before we can hope to aſcertain its cauſes. For this reaſon he earneſtly recommends it to his readers to avail themſelves of every opportunity that may occur to them of obſerving the phænomena of affections of this kind, and to mark with accuracy, in ſuch caſes, the temperature of the ſkin and urine, and likewiſe the ſtate of the pulſe and reſpiration.

XVII. *A Case of double Uterus* *. *By* Antonio Canestrini, *Physician to the Imperial Mines at Schwatz in Tyrol. Translated from the German.*

MARIA ANNA VOKON, wife of the Overseer of a Foundry at Fernetz, small of stature, but of a healthy constitution, was married in the twentieth year of her age, and before the end of a twelvemonth was delivered of a female child, which died two days after its birth. A year and a half afterwards she was delivered of another girl, which she suckled, but which lived only five weeks. During each of these pregnancies she was in good health, and in both went her full time. The menses never appeared during pregnancy, but at other times she was subject to them rather profusely, and they generally continued eight days.

* This curious case is extracted from the first volume of a respectable periodical work, entitled "Oberdeutsche Beyträge zur Naturlehre und Oekonomie, gesammelt und "herausgegeben von *Karl Erenbert von Moll*, Oesterreichischern Landmanne, der Gesellschaft Naturforschender "Freunde in Berlin, &c. Mitgliede." 8vo. Salzburg, 1787.—EDITOR.

Two

Two months after the birth of her ſecond child, being then in her twenty-fourth year, ſhe again became pregnant. In the fourth month of this laſt pregnancy ſhe fell down as ſhe was carrying ſome wood, and immediately felt much pain of her left thigh, but which ſoon ſubſided. A few days afterwards, however, when, according to her reckoning, ſhe had completed the fourth month of her pregnancy, ſhe was ſuddenly ſeized with pains of the belly, which made her cry out violently. This happened on the 19th of May, 1781. Her huſband immediately carried her to bed, and called in one of his female neighbours, who rubbed the abdomen with oil, but this did not at all mitigate the pains, which reſembled labour pains, though without being accompanied with any diſcharge from the vagina. In the evening of that day ſhe had a ſtool, after having been three days without ſuch an evacuation. The pains, however, ſtill continued; and ſhe was twice ſeized with vomiting. At ſix o'clock the next morning ſhe appeared to be in a dying ſtate, and about ſeven ſhe expired.

The relations of the deceaſed, ſtruck with the ſudden and fatal termination of the caſe, and obſerving that the pains had been confined

to

to the abdomen; that ſhe had twice been ſeized with vomiting, and that the abdomen ſwelled after her death, were ſuſpicious that her death might have been occaſioned by poiſon. They, therefore, reſolved to have the body opened, and I was called upon for this purpoſe.

After dividing the integuments from the ſternum to the pubis, and opening the cavity of the belly, I found therein a conſiderable quantity of extravaſated blood, of which about two pounds were removed before it was poſſible for me to examine the ſtate of the viſcera.

The omentum was of the uſual ſize, and moderately fat; the ſtomach exhibited no marks of inflammation, and the liver, gall bladder, and inteſtines, appeared to be in a ſound ſtate. In order to examine theſe parts properly, it became neceſſary to remove about three pounds more of extravaſated blood, and in doing this my hand met with a round and very elaſtic body, which was carefully extracted, and proved to be an ovum with its membranes entire. I next diſcovered that the uterus was lacerated on the right ſide, at its fundus, to the extent of an inch and a half; and in order to examine this part more accurately, I made an inciſion into it, from

from the place where it was torn to its neck, (ſee the reference at *g*. Fig. 1. Plate II.)

Upon laying open the cavity of the uterus I found the placenta ſtill ſo firmly adhering to its inner ſurface, that I was unable to detach it without tearing it. The uterus was three inches and a half long; its breadth, at its upper part, was two inches and a half, and at its lower part one inch and a half; near its neck it was of the thickneſs of a little finger; but at its fundus, where it was torn, it was not more than a fourth part ſo thick. Externally it was only ſlightly red, but internally it was much more ſo, and covered with innumerable ſmall veſſels and fibres; but it no where exhibited any appearance of gangrene. The laceration extended exactly acroſs the fundus uteri.

It had only one Fallopian tube, and that was on the right ſide, and contiguous to the laceration. Through its outer extremity I was able to introduce a probe, which paſſed into the cavity of the uterus. There was likewiſe only one ovarium, but it was larger than uſual, and weighed two drachms and twenty-two grains. The broad and round ligaments, which likewiſe were to be found only on this ſide, were of their natural ſize and figure. On the left ſide I

could diſcover no appearance of ovarium, Fallopian tube, or ligaments. This uterus did not ſeem to be connected with the vagina, nor was there any appearance of an os uteri, but it terminated in a ſort of round ligament, or neck, which in length was about a finger's breadth, but in circumference only of about the ſize of a little finger. At the upper end of this neck, where I had cut through it, I diſcovered two ſmall orifices, through which I could with difficulty introduce the probe I had before uſed for the Fallopian tube; but this paſſed too little a way to enable me to trace theſe channels to their termination. In other reſpects this neck appeared like the reſt of the uterus. A conſiderable number of blood veſſels paſſed through it to the body of the uterus, and in cutting through it I divided two of the ſize of a common writing pen.

This ſingularity of ſtructure could not fail to excite my aſtoniſhment, and a queſtion naturally aroſe in my mind, How had this woman twice been delivered, when the ſtate of the parts ſeemed to render delivery impoſſible? — That I might leave nothing unexamined, I carefully removed from the pelvis all that remained of extravaſated blood, and ſought for the

the place where I had cut through the uterus; and in ſo doing I found that I had only ſeparated it from another uterus, which I now took out, together with part of the vagina connected with it, and remarked what follows:

This ſecond uterus was ſix inches in length; at its upper part four and at its under three inches and a half in breadth, and about half an inch in thickneſs. Its appearance was natural, but its blood veſſels were empty, and its orifice was ſo firmly cloſed, that it was with difficulty I could introduce a probe into its cavity. The vagina was likewiſe in a natural ſtate. Here alſo there were only one Fallopian tube and one ovary. The former was connected with the uterus at its fundus, and on the left ſide, and was of the uſual ſize and appearance. The latter was ſomewhat flatter than the ovary of the other uterus, and weighed only one drachm and forty-two grains. The round ligament here was of the ſame thickneſs as that of the firſt, and the broad ligament was perfectly natural. On the right ſide there was neither ovarium, tube, nor ligaments.

Upon cutting open this uterus there flowed out between two and three ounces of a viſcid, reddiſh fluid; and its inner ſurface exhibited

the appearance of numerous and large blood veſſels, but they were empty. In future, to avoid miſtake, I ſhall call this the *larger* uterus, to diſtinguiſh it from the other in which the fœtus lay, and which, from its ſize, I ſhall ſtyle the *ſmaller* uterus.

On the right ſide of the neck of the larger uterus, an inch above the os uteri, was the place where the ſmaller uterus adhered to it, and which I had overlooked, and cut through. I now ſought for the origin of the two channels, the orifices of which I had obſerved in the neck of the ſmaller uterus, and which I now conjectured might paſs through this neck to the larger uterus. The clearing up this matter took me up ſeveral hours. I attempted to paſs firſt a probe, and afterwards merely a hog's briſtle, through this neck into the cavity of the larger uterus; but failing in theſe attempts, I cut through the middle of the larger uterus, in order the better to diſcover the communication. When I had done this, upon carefully examining the place on the inner ſurface of the cavity, immediately oppoſite the part where the outer openings were to be ſeen, I diſcovered, about an inch above the os uteri, a ſmall opening, through which I was able to introduce a

hog's briſtle. This paſſed from the cavity of the larger uterus to the neck of the ſmaller one; but ſtill the courſe of the other channel remained to be aſcertained, and this proved a ſubject of greater difficulty; for even quick-ſilver, with which I filled the larger uterus after carefully ſecuring the os uteri by ligature, failed to point it out, the quickſilver making its way out either through the little opening juſt now mentioned, or through the Fallopian tube. At length I examined carefully the outer ſurface of the orifice of the larger uterus, the form of which was perfectly natural; a portion of the vagina was ſtill connected with it, and as I was drawing this and one of the lips of the os uteri from each other, I diſcovered, at the part where the vagina embraces the latter, between the ſides of the vagina and the os uteri, a ſmall funnel-ſhaped orifice, which opened into the vagina. Its orifice was wide enough to admit the head of a large probe; but the channel ſoon became ſo narrow as to allow only a hog's briſtle to go through it. This paſſed under the outer coat of the larger uterus towards the neck of the ſmaller one, where I had before introduced a briſtle into it.

Fig. II.

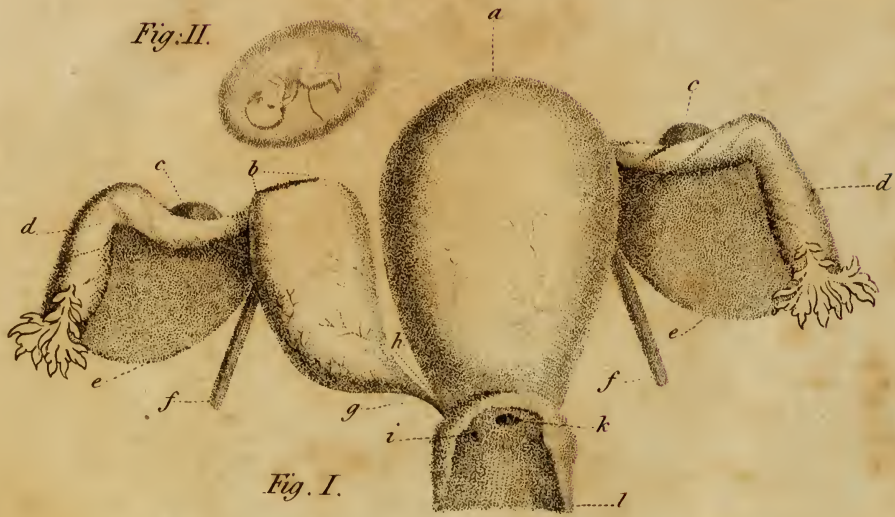

Fig. I.

The fœtus was a male, and ſeemed to be perfectly formed.

Explanation of Plate II.

Fig. 1.

a. The larger uterus.

b. The laceration of the ſmaller uterus, through which the fœtus eſcaped into the cavity of the abdomen.

c. c. The ovaria.

d. d. The Fallopian tubes.

e. e. The broad ligaments.

f. f. The round ligaments.

g. The neck or ſtem of the ſmaller uterus cut through by the knife.

h. The part at which one of the two ſmall channels that paſſed through the neck of the ſmaller uterus opened into the cavity of the larger one.

i. The orifice of the other ſmall channel, by means of which a communication was formed between the ſmaller uterus and the vagina.

k. The orifice of the larger uterus.

l. The vagina cut open lengthwiſe, anteriorly, ſo as to ſhow its communication with each uterus.

Fig. 11.

The fœtus ſurrounded by its membranes.

XVIII. *An Account of the Experiments and Diſcoveries of* Lewis Galvani, *Profeſſor of Anatomy at Bologna, relative to the Powers of Electricity in Muſcular Motion.—Vide* Aloyſii Galvani *de Viribus Electricitatis in Motu Muſculari Commentarium.* 4to. Bologna, 1791.

FROM a fact mentioned in a periodical work, publiſhed in the year 1786 at Bologna, relative to a Student of Phyſic, who, in diſſecting a mouſe alive, was much ſurpriſed, on touching the intercoſtal nerve with his ſcalpel, to experience an electric ſhock of conſiderable ſtrength in his hand, the Abbé Vaſſalli, Profeſſor of Natural Philoſophy at Tortona, conjectured that Nature had ſome means of accumulating and retaining electricity in ſome parts of an animal body, and of making uſe of it occaſionally. He was confirmed in this opinion by ſome experiments, an account of which he publiſhed in 1789; but Profeſſor Galvani, (to whom the Public are already indebted for ſeveral valuable communications * on different ſubjects of Anatomy) led on by an accidental circumſtance, has gone much farther, and has

* *Vide* De Bonon. Inſtit. Comment. Tom. V. et VI.

opened

opened to phyſiologiſts a ſource of new ideas relative to muſcular motion.

As he was diſſecting a frog in a room in which ſome of his friends were amuſing themſelves with electrical experiments, at the very inſtant he happened to touch a nerve of the frog with his knife ſome one drew a ſpark from a chain connected with the electrical apparatus, and the body of the animal immediately became convulſed.

The Profeſſor, attributing this effect to his having accidentally wounded the nerve, pricked it in reality, but excited no motion. He then touched it at the moment another ſpark was drawn from the electric chain, and the contraction again took place. He made the ſame experiment a third time, but without any ſuch effect. He now perceived that he held his knife by the handle, which was of bone, and, of courſe, a bad conductor. He repeated the experiment ſeveral times with different non-conducting ſubſtances, and no contraction enſued; but it conſtantly took place whenever metallic bodies were applied to the nerves.

He now faſtened to a nerve an iron wire of conſiderable length, and upon a ſpark being drawn from the electric chain the convulſive motions were renewed: he therefore called this wire the *nervous conductor*.

Inſtead of a wire he afterwards ſubſtituted an iron hook fixed to the ſpinal marrow of the frog. In ſome of his experiments he brought the frog itſelf near to the electrical machine, and in others the conductor alone, the animal itſelf remaining at a conſiderable diſtance, and in either way contractions were always produced. They were even obtained, we are told, by an inſulated conductor of more than two hundred feet long. They were found to be ſtronger if, by means of a conducting body, the feet of the animal were made to communicate with the earth; and under theſe circumſtances the phenomenon, it ſeems, was conſtant, whether the animal was inſulated or not.

The effect produced by conducting bodies, which communicated with the feet of the animal, led him to ſuſpect that conducting bodies applied to muſcles might occaſion this contractile motion. He therefore faſtened to a muſcle metallic wires, which he diſtinguiſhes by the name of *muſcular conductors*, but without effect, for, if the nervous conductor were wanting, no motion could be excited.

A frog, prepared for the experiment by removing its integuments, was laid on a non-conducting ſurface, on which the nervous conductor was ſo placed as to be at the diſtance of

ſeveral

ſeveral lines, or even an inch from the nerve. The moment a ſpark was drawn from the electrical apparatus the limbs of the animal contracted. The ſame thing happened, we are told, on placing the animal on a conducting ſurface, and the nerves with their conductor on a non-conducting ſurface. No difference was obſerved when the nervous conductor was covered through its whole length with ſealing wax.

The animal was afterwards placed on the magic picture, and a ſtrong ſpark was drawn, but without exciting any contraction.

All theſe experiments having been made with poſitive electricity, the Profeſſor was deſirous of ſeeing if the ſame effects would be produced by negative electricity.

With this view he inſulated a frog, prepared as in the former experiment, and a man. The latter drew ſparks from the ſurrounding bodies, and this produced the ſame effects as before on the animal. The ſame thing happened when he made the nervous conductors communicate with the negative ſurface of a Leyden phial, in whatever way the ſparks were drawn. He experienced the ſame effects, 1. with the electrophorus upon bringing the animal near the electrometer; 2. on placing the animals in veſſels, at a great diſtance, by means of ner-

vous conductors connected with the nerves that are distributed to the surface of the body.

He separated the crural nerves of a frog from the surrounding parts, and having removed them from the muscle, he applied the conductor to the latter, and when sparks were drawn from the electrical apparatus a motion was excited in the corresponding limb of the animal.

In all these experiments the animals had communicated, by means of the surrounding air, with the electrical apparatus. He therefore tried if there would be any difference in the result by interrupting this communication, or suppressing it altogether.

For this purpose the animal was placed under a glass vessel, and upon sparks being drawn was found to contract as before. Other vessels were placed over this, and the same contractions were observed to be weaker and weaker in proportion to the number and thickness of the vessels. The animal was even placed under the receiver of an air pump, and whether the air was drawn out or not, at the moment sparks were drawn, some degree of contraction was observable.

When the nervous conductors were brought near the electric chain, though no spark was drawn, the animal was strongly agitated.

Hitherto

Hitherto the experiments had been made on animals of cold blood. He now repeated them on fowls and ſheep; the reſults were the ſame; and from theſe experiments he is led to conclude, that the animals the beſt calculated for them are the older ones, and thoſe whoſe muſcles are the whiteſt. He has found that the fleſh of animals which have been ſubject to theſe trials corrupts ſooner than that of others.

After numerous experiments with artificial electricity, he was induced to have recourſe to natural electricity, drawn from the atmoſphere by means of a conductor fixed to the top of his houſe, and communicating with his chamber by a metallic wire connected with it. On this wire he ſuſpended animals of cold blood, and others, properly prepared, and by means of wires faſtened to their legs formed a communication between them and the ground. Every time there was lightning the animals were affected with ſtrong contractile motions, which preceded, and were obſerved to correſpond with the intenſity and frequency of the thunder. Even when there was no lightning ſimilar movements were excited as often as ſtormy clouds paſſed over the houſe; but when there was lightning,

lightning, with a ſerene ſky, the animals exhibited no appearance of contraction.

In theſe different experiments the Profeſſor had conſidered only the electricity which is extraneous, as it were, to the bodies of animals; but in the courſe of his inquiries an accidental circumſtance directed his attention to the electricity which is peculiar to, and inherent in, animals.

He had ſuſpended, by means of metallic hooks fixed to their ſpinal marrow, ſeveral frogs on an iron balcony in his garden, and had repeatedly obſerved that theſe animals gave ſigns of contraction. At firſt he thought this might be owing to ſome changes in the atmoſphere: but a more accurate inquiry convinced him he was miſtaken; for having placed in his chamber, on an iron plate, an animal, properly prepared, with the hooks fixed to its ſpine, on preſſing it againſt the plate he ſaw, with ſurpriſe, the ſame motions he had obſerved in the animals ſuſpended on the balcony. Making uſe of different metals, he tried them at different times, and on different days, and always with the ſame reſults, except that the contractions varied according to the diverſity of the metals. He found that ſilver was better than

any

any of the others for theſe purpoſes. He made ſimilar experiments with non-conducting bodies, but always without ſucceſs. Hence he began to ſuſpect that the animal had really an electricity peculiar to itſelf. This ſuſpicion was confirmed when he found that the circulation of the nervous fluid from the nerves to the muſcles, at the time the phenomenon happens, is nearly ſimilar to the circulation of artificial electricity in the Leyden phial. The following fact led him to this diſcovery:

While he was holding with one hand, by means of a hook, a prepared frog, in ſuch a manner that its feet touched a ſmall ſilver baſon, he happened accidentally with his other hand to touch the baſon; violent contractions immediately took place in the whole body of the animal, which were renewed every time the ſame mode of communication was repeated. When one perſon held the frog, and another touched the diſh, the animal remained immoveable.

Having thus perceived the neceſſity of a communication to excite motion, he engaged in freſh experiments on this ſubject. He placed, on a non-conducting ſurface, a prepared frog, and applied one end of a bent wire to the hook connected with the nerve, and the other end to the

the feet, or to the muſcles of the legs of the animal, and there was immediately a contraction. If the bent wire was interrupted by a non-conducting ſubſtance, the frog remained motionleſs.

When a frog, properly prepared, was ſuſpended by the extremity of the foot, if the hook touched a metallic plate, and at the ſame time the other leg of the animal touched this plate, this leg immediately contracted.

The Profeſſor found that different metals were productive of different effects in theſe experiments. If the plate, the hook, and the bent wire, were all of iron, the motion was more feeble, or even altogether wanting; but if one of theſe was of iron, and the reſt were of copper, or, what anſwered ſtill better, of ſilver, the contraction immediately took place, and continued a much longer time.

As the circulation of the Leyden phial ſuppoſes two contrary electricities, the one more condenſed, or poſitive, and the other leſs ſo, or negative, ſo Profeſſor Galvani concludes, that a ſimilar diſtinction takes place in the bodies of animals, and that one of theſe electricities, viz. the condenſed, or poſitive, is ſeated in the nerves, and the other in the muſcles.

He

He was led to theſe concluſions by the following experiments:

He applied, in ſome inſtances, glaſs cylinders, and in others ſealing wax, to the ſpinal marrow of frogs: with the former he obtained no motion in the animal, but the latter conſtantly excited it. If the ſpine of the frog was covered with tin foil, the ſealing wax, though at the diſtance of the third part of an inch, or more, excited muſcular contraction; and upon bringing the frog near the electrical machine, and turning the latter ſeveral times, the animal exhibited no ſign of motion. This proves, he thinks, that the electricity of the nerves is poſitive.

He made, in the ſame manner, experiments to aſcertain if it were poſſible to excite motion in the muſcles; but in theſe no ſuch effect took place.

He next coated a nerve with tin foil, and obtained ſtrong movements upon touching this coating with different bodies. Similar effects took place when the brain was coated in like manner; but when the muſcles were coated, inſtead of the nerves, the animal remained immoveable, or afforded only very feeble ſigns of contraction.

Profeſſor

Profeſſor Galvani endeavoured to aſcertain whether the electricity is propagated from the nerve through the whole nervous ſyſtem, or is confined to the nerve which is the ſubject of the experiment; and he found that when the nerve is not ſeparated, but only laid bare, the electricity ſpreads itſelf through the whole body.

To prove that theſe phænomena are really the effects of electricity, he had recourſe to Dr. Franklin's magic picture, ſo that the nerves touched one of its ſurfaces, and the muſcles the other, and upon applying the conductor a very ſenſible contraction took place.

XIX. *Two Letters on Animal Electricity. By* Euſebius Valli, *M. D. of the Univerſity of Piſa.* — Vide *Journal de Phyſique.* 4to. Paris, 1792.

THE facts relative to animal electricity, deſcribed in the preceding article, appeared to Dr. Valli of ſo much importance, that he was induced to repeat the experiments of Profeſſor Galvani, and to inſtitute many new ones on the ſame ſubject, which ſerve conſiderably to illuſtrate

trate and extend the diſcovery in queſtion. The following account of his reſearches cannot fail, therefore, of being highly acceptable to our readers; and we give it to them with the more ſatisfaction, as the candid and ingenious author, who is at preſent in London, has more than once gratified us with a repetition of many of his experiments:

Experiment I.

The author opened the belly of a frog, ſo as to lay bare the ſpine, and diſcover the crural nerves that iſſue from it. Two lines above their inſertion he cut the frog in two, and, paſſing his ſciſſars under the origin of thoſe nerves, removed the remainder of the vertebral column, leaving only the vertebra that united the bundle of nerves. He ſurrounded this vertebra with a piece of thin lead, which formed a coating, and he removed the integuments of the lower part of the frog, ſo as to lay bare the muſcles. Having thus prepared it, he touched at the ſame time with an iron wire, diſpoſed as a conductor, and inſulated (by means of a piece of ſealing wax, which ſerved as a handle to it) the leaden coating and the muſcles of the frog, and he obſerved all the phenomena

phenomena described by Professor Galvani.—These phenomena, he tells us, take place equally whether the animal be insulated or not. He employed conductors of different metals, having observed that by so doing all the electrical phenomena become more apparent. Those of silver seemed to him to be the best.

Experiment II.

Two frogs, prepared in the manner above described, after having ceased to give any signs of life, experienced a very great tremor when touched by the conductor.

Experiment III.

While he made these experiments on one frog, he left another at rest that he had prepared at the same time. When the first had ceased to move, and was entirely extinct, he took the second, which in an hour and a half had lost none of the faculties that he had exhausted in the first, and, notwithstanding this delay, he made on this the same experiments as with the other, and obtained the same results.

Experi-

Experiment IV.

He had a frog, the crural nerve of which, and the extremity of the body correſponding with it, ſhewed no ſigns of feeling. On inquiring into the cauſe of this, he found that the filaments of the nerve were ruptured. Having brought them together, he coated them at the point of reunion, and, on applying the conductor, a tremor was excited in the leg. When this motion had ceaſed he divided the oppoſite nerve, and after collecting the filaments, and placing them at a diſtance from the limb, touched it ſeveral times with the conductor, but without exciting any movement.

Experiment V.

He prepared two other frogs, taking care to ſeparate the nervous filaments of each crural nerve. In making the experiment they were as much agitated as thoſe whoſe nerves remained in their natural poſition.

Experiment VI.

After having fatigued for an hour and a half two frogs, prepared as uſual, he left them at reſt an hour and ten minutes. He then at-

tempted to excite motion with a conductor made of copper covered with ſilver; one of them ſprang from the plate of glaſs on which it was placed, and remained afterwards twenty minutes affording only ſlight tremors. The firſt movement in the other was leſs violent, but it was nevertheleſs much agitated, and continued to be ſo full as long as the other.

Experiment VII.

Wiſhing to aſcertain how long frogs are capable of ſupporting this ſtate, he prepared two at ten o'clock at night. At ſeven the next morning he found them feeble, but not without motion. Both of them, on being ſubjected to the uſual experiment, afforded a ſlight tremulation. An hour afterwards they ceaſed to give ſigns of electricity to any of the means that were employed.

Experiment VIII.

At other times he has left, in the ſame manner, during the night, frogs prepared as uſual, but in the morning has found them dry, and yielding no ſign of electricity.

Experiment IX.

After ſeparating ſome muſcles from the body of a frog, and lacerating them, it was not poſſible to excite their irritability by a mechanical ſtimulus, but the conductor excited it.—"Does "the motion of the muſcles produced by the "irritation thus excited, or by the nerves that "are diſtributed to them," aſks our author, "differ from that which reſults from the diſ-"charge of electric matter? And which of "theſe motions approaches the neareſt to the "voluntary motions?"

Experiment X.

The brain of a frog having been laid bare, and irritated, it died apparently in convulſions; but on applying the conductor the muſcles of the animal immediately contracted.

Experiment XI.

The preceding experiment, our author tells us, was repeated in order to compare it with what would happen to frogs dying without convulſions; compariſon, he remarks, being the rule we ought to follow when we have no other that is better or more accurate. No difference

was perceptible; and therefore he concludes that the animal loſt nothing in the convulſions, and that the principle of life was preſerved. But a man, he obſerves, agitated by convulſions and nervous affections is exceſſively debilitated. This leads him to offer a query, which he thinks we ſhall ſome day be able to ſolve — "Does "there exiſt in the animal œconomy another "agent beſides electricity?"

Experiment XII.

He applied opium to one of the crural nerves. The limb ſeemed to ſuffer by it a little, as did alſo the other limb; but after ſome time both recovered their former vigour.

Experiment XIII.

Opium applied to the extremity of a divided nerve produced no apparent diminution of electricity in the part.

Experiment XIV.

After having kept for ten minutes one of the extremities of a prepared frog in a tepid bath of opium, the limb, on being ſubjected to the uſual experiments, became fatigued in leſs than a quar-

a quarter of an hour, and when it ſeemed to have loſt its electricity he proceeded to the other limb, which moved vigorouſly on applying the conductor, and gave ſigns of electricity for at leaſt an hour and a half.

Experiment XV.

Three frogs were made to ſwallow a ſolution of opium in warm water; and at the end of an hour were prepared, and placed in a ſolution of opium; notwithſtanding which they ſtill continued to move when excited.

Experiment XVI.

After bathing with a ſimilar ſolution the adductor and triceps muſcles of the thigh of a frog, their motions ſeemed to be ſtronger than uſual; but the author thinks that this might be accidental.

Experiment XVII.

A ſolution of opium was poured between the ſkin and the muſcles of the thighs of two frogs, but without impairing their electricity.

Experiment XVIII.

A ſolution of opium was introduced between the fibres of the triceps muſcle of a frog, the extremities of which were already impregnated with the ſame ſolution. This frog remained immoveable.

Experiment XIX.

In ſix other frogs the phenomena were different, the opium having neither weakened nor ſuſpended their electricity.

Experiment XX.

Opium applied to the inſulated muſcles of frogs extinguiſhed their electricity only in one inſtance out of twenty, but in that one the effect, we are told, was inſtantaneous. Theſe facts, the author obſerves, embarraſs him.

Experiment XXI.

The muſcles of living frogs ceaſed to be excited by mechanical ſtimuli after opium had been applied to them, or to their nerves, and yet were excited by the conductor as often as it was employed.

Experiment XXII.

The author laid bare the brains of four frogs, and applied opium to them. They fell down as if ſtruck with lightning. He prepared them for the experiments, leaving the lower extremities united to the trunk and the head. The ſpine was cut off, and ſeparated below the crural nerves. Upon coating them, and applying the conductor, the uſual phenomena enſued.

Experiment XXIII.

Inſtead of opium, other extraneous ſubſtances were applied to the brains of different frogs, but without ſeeming in the leaſt to impair their electricity.

Experiment XXIV.

Six frogs were made to ſwallow a conſiderable quantity of opium; but in none of them was the electric principle apparently weakened by it.

Experiment XXV.

Tobacco, in powder, rendered five frogs profoundly ſtupid and inſenſible; nevertheleſs they gave the uſual ſigns of electricity with the conductor.

Experiment XXVI.

The author coated the nerves of the legs of lizards, and obtained ſlight contractions; but on coating the marrow of the tail the motions he excited were more violent, and laſted longer.

Experiment XXVII.

Lizards that had been poiſoned with tobacco, and died in convulſions, were found not to have loſt their electricity. In numerous experiments to aſcertain this fact, not one, we are told, was contrary to it.

Experiment XXVIII.

The author coated, near the head, the ſpinal marrow of two tench, each of which weighed about an ounce and a half. They raiſed their fins five or ſix times when excited, but in leſs than two minutes ceaſed to yield any motion.

Experiment XXIX.

An eel was cut in two, and the ſpinal marrow of each of the two portions was coated with tin foil. On applying the conductor the tail ſtruck violently, as if it had been in water; and on continuing to touch it it turned different ways; but

but inſenſibly grew weaker, and in leſs than three quarters of an hour its electricity was extinct.

This principle was leſs ſtrong in the part next the head, but it laſted about five minutes longer.

Experiment XXX.

The wing of a lark, prepared in the uſual way by laying bare the muſcles, experienced ſlight movements for three minutes, but the legs could not be excited. The author aſcribes this failure to the ſmallneſs of the crural nerves in this bird.

Experiment XXXI.

A kitten, juſt born, was carefully prepared, and contractions were excited in it for ſeven or eight minutes; but no movement could be produced in the muſcles of the tongue or larynx.

Experiment XXXII.

The author prepared two dogs. The firſt, from a want of proper precaution in the experiment, afforded nothing; but the ſecond, which he killed by a blow of the head, yielded ſtrong motions, and in particular one of its fore paws bent

bent five or ſix times, as in walking. The hyogloſſi and geniogloſſi muſcles trembled ſeveral times. Thoſe of the larynx alſo, the nerves of which had been coated, were affected in the ſame manner.

The heart did not palpitate, although the eighth pair of nerves were coated while that viſcus was reeking and warm. In an hour all was over.

We now come to the author's ſecond letter.

From ſome of his firſt experiments he had been led to aſſert that a ligature paſſed round a nerve prevents the paſſage of the electric fluid; but one of his friends (Mr. Fattori) having informed him that this is not always true, he repeated the experiment, and found that by making the ligature on the nerve cloſe to its inſertion into the muſcle the motion is entirely prevented; but that, on the contrary, if the ligature be at a diſtance from the muſcle, the experiment ſucceeds very well.

There is no part of an animal, he obſerves, that is not a conductor of electricity, and he is unable to ſay which is the beſt, becauſe he has ſeen an infinite number of anomalous appearances.

The

The ſhocks, he aſſures us, are, in general, ſtronger if the conductor is carried from the muſcles to the coated nerve, inſtead of its being carried from the latter to the muſcles. — If this laſt method is adopted, when the electricity is ſo weak that it is nearly extinct, no motion enſues; yet even then it may be excited by the other proceſs. This fact, as the author obſerves, is ſingular, and merits the attention of philoſophers.

Slight wounds of the brain in frogs, ſometimes, we are told, occaſion convulſions or palſy, and at other times produce no ill effect.

When the wound of the brain is more conſiderable, the animal ſometimes dies ſuddenly; but the author has ſeen frogs ſurvive for ſeveral days the deſtruction or laceration of this organ.

In general, however, he has found that frogs whoſe brain has been lacerated have yielded electric movements only for about two minutes. He ſuſpected that this might be owing not to the want of electricity, but to the nerve becoming a non-conductor, or to ſome change in the ſtate of the muſcular fibre: and having in one inſtance obtained movements by ſubſtituting, in the place of the nerve, a very ſmall and highly-poliſhed iron wire, and in another obſerved

obſerved a manifeſt alteration in the ſtate of the muſcles, his opinion on this ſubject was rendered leſs doubtful.

Frogs that he had deprived of their electricity, by means of the conductor, corrupted ſooner than others which had not been deprived of it. In a note to this paſſage the author aſks, Am I not miſtaken? Repeated diſcharges, do they really deprive the animal of its natural electricity? or do they only put it in equilibrium? I know not, he ſays, which of theſe to believe. It is certain, however, he adds, that frogs fatigued by the conductor, particularly in water, ſoon become putrid. What a curious diſcovery, he obſerves, it will be if it ſhould hereafter be aſcertained that the electric fluid retards the putrefaction of bodies. Before the preſent diſcovery he knew, he tells us, that the fluid which circulates in the nerves is a powerful antiſeptic.

Several frogs killed by the diſcharge of the Leyden phial gave the ſame ſigns of electricity as others that had not experienced ſuch a ſhock. The ſucceſs of this experiment, however, the author obſerves, muſt depend on care being taken that the diſcharge be not ſtrong enough to diſorganize the whole machine.

Frogs

Frogs live ſeveral days in confined air without their electric quality ſeeming to ſuffer. Neither inflammable nor nitrous airs have been found to affect it; but it ſeemed, we are told, to experience a little diminution from phlogiſticated air; and it was much injured by air vitiated by the combuſtion of ſulphur. This effect, however, the author obſerves, was leſs perceptible in frogs that were only expoſed to it after being prepared, than in ſuch as had breathed and died in it. Under ſuch circumſtances the muſcular fibres were ſometimes found in a relaxed, and at others in a rigid and tenſe ſtate; and in the experiments the ſhocks were very weak, and after a few ſeconds could not by any means be excited. Is it a portion of electricity, the author aſks, which in this caſe is diſſipated? or is it the fibre which has loſt its natural ſtrength?

He has found that inflammable air extinguiſhes the life of a linnet or a Canary bird, but not their electricity, although it is naturally very weak.

He killed two kittens in phlogiſticated air; and having prepared their fore legs, found the uſual ſigns of electricity.

A dog

A dog was made to ſwallow arſenic, and died, but the poiſon was not found to have weakened his electricity. In other experiments hemlock was employed, and the reſult was the ſame.

If experiments ſhould ſhow that poiſons do not leſſen the electricity of animals, or, more properly, the capacity of parts to contain it, it will be neceſſary, our author thinks, to inquire why poiſoned animals corrupt more ſpeedily than others? It muſt be another principle of life, he obſerves, that has been offended. But where, he aſks, does it exiſt? Probably in the nerves, ſince miaſmata and venomous ſubſtances exert on them their firſt action. But our data on this ſubject are, he acknowledges, as yet too few to reaſon on.

Some frogs expoſed to the exhalation of putrid fleſh retained, after their death, weak ſigns of electricity.

Frogs killed by being placed under the exhauſted receiver of an air pump, and afterwards ſubjected to the uſual experiment, exhibited only feeble contractions, and theſe were excited with difficulty. In all of them an extravaſation of blood had taken place in the cellular membrane of the muſcles, ſo as to render their fleſh of a lively red colour. The blood being a conductor

tor of electricity, it in this caſe, the author obſerves, had diſperſed a portion of it at the expence of the nerves, which are the road the fluid takes to reach the muſcles. On repeating the experiment with prepared frogs no ſuch effuſion was produced, and the electricity ſhowed itſelf pretty well. — For theſe two experiments the author acknowledges himſelf indebted to Mr. Moſcati.

To a friend who had undertaken to explain, by means of the Leyden phial, all the phenomena of animal electricity, our author, after pointing out the difficulties of ſuch an hypotheſis, offered his own theory on the ſubject, which is as follows:

The electric fluid, ſays he, is either ſent from the ſenſorium commune to the muſcles through the nerves, or returns to the ſenſorium through the infinite ramifications of theſe ſame nerves from the whole ſurface of the body, or is diffuſed through the body according to certain laws. In a word, electricity acts in the body in the manner phyſiologiſts have ſuppoſed the nervous fluid to do.

To confirm this theory he contrived ſeveral experiments; among which the following, he thinks, appears of ſome weight:

He took a frog, and, after removing its integuments, laid bare the vertebral column, which he divided above the origin of the crural nerves, and alſo at the origin of the lower extremities. The animal was thus divided into two parts, which communicated with each other only by the crural nerves. He coated theſe nerves, and placing one of the branches of the conductor on the coating, and the other on the trunk, the lower extremities were at that inſtant agitated as well as the upper parts and the fore feet.

If the experiment be repeated by tying the nerve, there is, he obſerves, no movement in the lower extremities.

If inſtead of placing the conductor on the trunk, it be placed on the ovaries, liver, lungs, head, or feet, the phenomenon, we are told, takes place equally. In this caſe, ſays the author, we do not eſtabliſh a communication between the external and internal ſurfaces of the muſcles, which are below the coating, and which notwithſtanding exhibit movements: it is the electric ſtream that paſſes from above downwards. Profeſſor Galvani himſelf, it ſeems, had remarked, that on making the experiment in a contrary direction the electricity of the lower extremities moved upwards; conſe-

quently, obſerves our author, the electric fluid circulates between the nervous filaments in all ſorts of directions. This, he thinks, is much in favour of his theory.

It is eaſy, he remarks, to prove that, without increaſing the degree of electricity, we may increaſe its celerity, as appears from the following experiment:—He took a frog, prepared as uſual, and directed againſt it a ſtream of electricity by means of a chain that communicated with its nerves. The animal, which at firſt was agitated, became after ſome little time immoveable. When it was in this ſtate, upon removing the conductor a little the frog renewed its motion, but ſoon fell into its former ſtate of inactivity. The current of electricity was then accelerated by applying an inſulated conductor to the muſcles of the frog, and the animal immediately began to move again. When it again ceaſed to move, by communicating with the conductor, he was again able to excite movements.

This ſhows, he thinks, that electricity is always the ſame, and that we only vary the manner of applying it.

We are not to ſuppoſe, however, he obſerves, that the ſame thing happens preciſely to the animal in full life. But he is convinced that

there exiſt in the animal cauſes capable of retarding or accelerating the electric current. He thinks we muſt ſeek for theſe cauſes in the different modes of ſenſation to be obſerved in the nerves; in the different properties of their cortical and medullary ſubſtances; and perhaps alſo in another principle, which exiſts in the nerves along with the electric fluid, and which is more or leſs combined with it. The ſubject, he is aware, is full of obſcurity. We ſhall, he obſerves, perhaps never ſee it clearly diſplayed, or, if we do, it will not be till after long and immenſe reſearches, and probably not till after we have been indulging in many erroneous ideas concerning it.

One great ſtep, however, he contends, is already made. The exiſtence of electricity has been demonſtrated in the animal machine; and this important diſcovery, he thinks, will tend to the explanation of many phenomena, among which he contents himſelf with mentioning the following:

Man and animals live a long time without refreſhing the blood with freſh chyle. If the blood were the fund which ſhould furniſh the principle that animates all the parts, and without which no movement or function of the ani-

mal

mal œconomy could be executed, ſo great a waſte of life could not be of long duration. But now, he thinks, the myſtery is unfolded. The animal who takes no food attracts from the earth, and from the atmoſphere, this precious and neceſſary principle, the electric fluid.

It having been ſuggeſted to our author, that, in order to determine whether the nervous fluid be really the electric fluid or not, he ſhould have recourſe to the electrometer; and not happening at that time to be poſſeſſed of one ſufficiently ſenſible, he made the following experiment:

He prepared ſeveral frogs, the crural nerves of all which he united in a ſingle coating. Having put in order this battery, and eſtabliſhed a communication, by means of a conductor, between the nerves and the muſcles, he excited the electricity, and conſequently the movements. At the moment of the diſcharge two very ſmall bits of ſtraw, at a little diſtance the one from the other, but almoſt touching the apparatus, immediately approached each other. This experiment, he obſerves, proves the ſame thing as the electrometer would have done; but he has ſince, it ſeems, (as we are told in a note) em-

ployed the electrometer, and found that it gave ſigns of electricity.

On the day he wrote this account he, for the firſt time, he tells us, coated the muſcles inſtead of the nerves, and in this way obtained ſtrong movements; but of this he propoſes to ſpeak more fully hereafter.

XX. *Additional Obſervations on Animal Electricity. Communicated in a Letter to* Samuel Foart Simmons, *M. D. F. R. S. by* Euſebius Valli, *M. D.*

SINCE the publication of my two letters in the *Journal de Phyſique*, I have made a conſiderable number of new experiments on animal electricity, many of which you, my dear Sir, and ſeveral of your friends, have had the goodneſs to ſee me repeat, on different days, at your houſe and other places. Theſe additional facts are deſcribed in a ſeries of letters on the ſame ſubject, which before I left Paris I put into the hands of our worthy friend M. De la Metherie, in order to their being inſerted in his valuable Journal. But I ſhall here give you the principal reſults of theſe, and of ſome other ſtill later

later inquiries, to be added, if you think proper, to the account of my firſt experiments, which I find you mean to do me the honour of inſerting in the volume of Medical Facts and Obſervations about to be publiſhed.

I now, as you have repeatedly witneſſed, obtain muſcular movements by coating the muſcles inſtead of the nerves.

Movements may likewiſe be excited by eſtabliſhing a communication between muſcle and muſcle, or between nerve and nerve.

By uniting in one coating the nerves of ſeveral frogs, and exciting their electricity, ſome fat's hairs, placed for this purpoſe in the apparatus, were obſerved to recede from and approach each other.

Even in the living frog I am able ſometimes to excite electricity, in a very ſenſible manner, by employing two coatings, one under the belly, and the other on the back.

The different metals, whether employed in the way of coating, or as conductors, afford remarkable differences in their effects. With gold and ſilver, for inſtance, the experiment will, in general, not ſucceed, if one of them be uſed as a coating, and the other as a conductor, though either of them will ſerve as a good con-

ductor when employed with another metal. I have, however, ſometimes excited movements in a pullet's wing with ſilver and gold.

What has been obſerved concerning the different metals will be found to hold good with reſpect to different fluids alſo; ſome of which, as, for inſtance, water, will be found to be good conductors; while others will conduct leſs perfectly, or not at all. It is an obſervation of Profeſſor Galvani, that oil, which, as we know, is not a conductor of common electricity, is likewiſe not a conductor of animal electricity.

The experiment in water may be made in the following manner: — The legs of a frog being prepared in the uſual way, and the crural nerves coated, two glaſſes filled with water are to be placed cloſe to each other. The legs of the animal are to be put into one of theſe glaſſes, and the nerves with their coating muſt hang over ſo as to be immerſed in the water in the other glaſs. You muſt now, with a piece of ſilver, or ſome other metallic conductor, touch the coating of the nerves, while with your other hand you touch the water in the other glaſs, in which are the legs of the frog; thus forming a communication between the two ſurfaces of the muſcles,

cles, and violent movements will inſtantly take place. If, inſtead of touching the nerves with the conductor, you touch them only with your bare hand, and put the conductor into the water in the other glaſs in which are the legs of the frog, no motion will be excited; but if the conductor be made to communicate with the water in both glaſſes, it is not neceſſary for the ſucceſs of the experiment to bring it into contact either with the nerves or with the muſcles. When the electricity of the animal is weak, a conductor made of a ſingle metal is not always ſufficient to excite it; and it then becomes neceſſary to have recourſe to a conductor made of two metals, as, for inſtance, of iron and ſilver.

The membranes of the nerves are bad conductors.

The different intenſity of electricity in different animals will certainly occaſion a great difference in its effects.

I have ſometimes obſerved that frogs have ſpontaneous motions ſtronger than thoſe excited by the metallic conductor; at other times the conductor has failed to produce any movement; but to inveſtigate all the various phenomena of animal electricity would require more experiments than I have at preſent leiſure to undertake.

This principle has probably a great ſhare in the production of animal heat.

Frogs killed by being plunged into hot water, and immediately taken out of it and prepared, have given ſigns of electricity; but if the limbs of the animal, after being prepared, be placed in very hot water, the experiment does not ſucceed.

The vitriolic and nitrous acids applied to the nerves, or rather to the muſcles, did not prevent the movements.

Opium applied to one of the crural nerves deprived the animal of the power of moving the correſponding leg; but did not prevent movement in it when the conductor was applied to it.

Inflammable, nitrous, and phlogiſticated airs act in different manners both on the living frog and on muſcles that are expoſed to them.

The wings of a pullet, prepared for the experiment, after having given ſeveral ſhocks, remained immoveable, notwithſtanding I continued to apply the conductor; but upon my irritating the nerve ſpontaneous motions immediately took place.

Fowls have little ſenſibility; yet their muſcular power ſeems to be conſiderable. Perhaps the

the irritability of animals is in an inverſe ratio to that of their ſenſibility.

Fowls killed by drowning have, in general, afforded me ſigns of electricity; and ſome of them, after having been apparently deprived of life in this manner, have been reſtored by exciting their electricity.

Animals that die of hunger have no ſenſible electricity.

Fowls that have died after inflammation or gangrene of the inteſtines have afforded no ſigns of electricity.

The equilibrium of animal electricity is not always broken, notwithſtanding circumſtances which ſeem favourable to ſuch an effect. My experiments on this head are deciſive. I will here mention one of them. I prepared the limb of a frog, and after coating the nerve with lead, placed it on a half-crown piece. At firſt I obſerved ſome movements, but afterwards the limb remained immoveable. By ſhaking the table on which the apparatus was placed, the ſhocks were renewed; but after this, in order to obtain any movement, it became neceſſary to ſhake the coating itſelf. This experiment ſucceeds likewiſe in water.

I have

I have reaſon to think that the nerves contain another fluid which i the vehicle of electricity. My conjecture on this head is founded o different experiments. For inſtance, after having coated a nerve, and apparently exhauſted the electricity of the correſponding limb, if the coating be removed a little higher up to a freſh part of the ſame nerve, movements will again be excited. This ſeems to ſhow that the vehicle only of the electricity in the part, and not the electricity itſelf, is exhauſted.

October 11, 1792.

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8. A Plan

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